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ARTICLE I.

ECLAMPSIA. PUERPERAL CONVULSIONS. By H. Z. GILL, A.M., M.D., Physician to the Southern Illinois Penitentiary at Chester, Ill. Read before the Jersey County Medical and Surgical Society.

The comparative frequency of puerperal convulsions is not agreed upon by obstetricians of different countries, but varies from one in two hundred to one in four hundred cases of labor, when taking large numbers of patients.

The convulsions may appear at any period of gestation, from the first month of pregnancy to the last, and even during the month following delivery. The last two months of pregnancy form the most susceptible period for the appearance of the disease. According to most statistics the following is given as the order of frequency as to the period of first attack: During labor, following delivery, and during pregnancy. But M. Bailly,* from whom I shall draw liberally, affirms that this is not the exact state of the

* Nouveau Dict. de Méd., et de Chirurgie Pratiques. Tome 12, Art. Eclampsie.

facts. He says, generally it is during the course of the ninth month that the convulsions set in, and that labor is the consequence of the attack, hence he changes the order to the following: Pregnancy, labor, following labor.

Symptoms—Prodromic.—Occasionally the attack may come on without any warning being observed by patient or attendants. This has even been denied, and has been charged to inobservance. There are three prominent symptoms giving warning of an attack of puerperal eclampsia (as well as frequently others of less significance), namely—pain in the head, disturbed vision, and epigastric pain or oppression. Other symptoms, less common and less pronounced, are—vomiting and dyspnœa, disturbance of intellection and of sensation, vertigo, tinnitus aurium, drowsiness or sleeplessness, and excitability.

Symptoms of the Paroxysm.—The paroxysms are remarkably similar in their general characteristics. The difference is mainly in degree of severity and duration. When once seen, the picture will be sufficiently familiar, ever after, in all its principal points. A brief description may be admissible. After having experienced for a short time the symptoms above described, the patient seems more absorbed and sunk in profound meditation, then her gaze becomes fixed for a few moments, and the paroxysm commences immediately by rapid contractions of the muscles of the face, the eye-lids and the eye-balls, which latter are seen to roll about in their sockets. These jerking movements, which give to the countenance a distorted expression, most painful to witness, quickly give place to tonic contractions of the same part and of the neck. The mouth at first is strongly drawn to the left, and the face is slowly drawn towards the shoulder of the same side. The eye-balls, turned upwards by their levator muscles, leave exposed but a small inferior segment of the sclerotic between the partially opened lids. After being slowly turned toward the left, the face, by a slow movement in the opposite direction, is turned toward the right shoulder. The convulsive agitation is rapidly extended from the head to the body. The extensor muscles of the trunk, powerfully contracting, tend to curve the spine backward (opisthotonos.) The entire body becomes ex-

tremely rigid. The extremities are equally rigid and are generally extended. The hands, forcibly pronated, are closed, the thumb in the palm of the hand, and the fingers closed tightly over it. More rarely the predominant action of the flexor muscles has the effect of immobilizing in a semi-flexed position the different parts of the upper extremities.

The diaphragm itself and the muscles of respiration finally become implicated. The respiration becomes suspended, and the face congested, flushed and purple. The tongue, projected from the mouth at the onset of the paroxysm, is seized and lacerated by the spasmodic gnashing of the jaws, and the blood which escapes from the wounds thus made gives to the mucus which is discharged upon the cheeks, a bloody tint. The muscles of the larynx, and perhaps those of the throat, strongly convulsed, close more or less completely the orifice of these cavities. The result is that the air, compressed by the convulsive contractions of the chest, escapes only with difficulty, producing a peculiar wheezing often quite marked. At the same time there is a complete suspension of intellection and sensation. The patient neither sees, hears, nor feels. An external irritant does not seem to be recognized.

Immediately after these tonic convulsions, those of a clonic character follow, which, like the former, extend to the entire muscular system. The general rigidity of the preceding period is followed by jerking movements, which occasion rapid agitation of the head, trunk and extremities. The result is, the most horrible grimaces are produced by the irregular movements of the mouth, the eyelids and even the eye-balls. Inspiration, until this time completely suspended, begins to be re-established. An interrupted and stertorous respiration produced with noise, expels bronchial and bloody sputa from the mouth. The agitation of the trunk and extremities consists of limited jerks which shake the body without displacing it; so that it is never necessary to hold the patient by force to prevent her from injuring herself as is the case in hysterics.

The cutaneous and visceral congestion caused by the interruption and disturbance of the respiration, increases during the first

period. It reveals itself at the periphery of the body by the injection of the ocular conjunctiva and the subconjunctival ecchymosis, by cutaneous discoloration, by the heat of skin and finally by an abundant perspiration which inundates the entire body. The pulse, full and strong at the beginning of the convulsions, becomes rapidly accelerated under the influence of the muscular and respiratory disturbance, and finally acquires an extreme frequency, at the same time that it becomes so weak as to be almost imperceptible, when the convulsion has attained its last stage. All these symptoms become ameliorated toward the end of the paroxysm. The respiration and circulation, at first interrupted or disturbed, becomes regular; the congestions, both superficial and deep, disappear; the cutaneous discoloration becomes natural again; the jerking of the extremities and trunk grows more feeble and farther apart, and at last cease entirely to give place to a more or less complete restoration.

Résumé—Two well defined stages or periods exist in the convulsive manifestations of eclampsia. The first, the tonic convulsion, lasts about twenty seconds. The second, the clonic, which succeeds the former, is much longer and generally continues from one to five minutes, and may even exceed the latter duration. The gradual re-establishing of the respiration during this period explains the fact that it may continue so long without producing death. The period of tetanic convulsions alone approximately threaten the life, and the cases in which the patients have died during the paroxysm are very likely owing to the uninterrupted duration of the tonic convulsions.

Symptoms—During the Interval.—Coma, more or less profound, and more or less prolonged, follows the paroxysm, with loss of intellection and sensation.

The approximate cause of the coma is not doubtful. It depends upon a violent cerebral congestion, and sometimes upon a serous effusion into the brain, the existence of which is sufficiently clear by the comatose symptoms during life as well by the anatomical proof on autopsy. This congestion is consecutive to the convulsion, and is the result of the obstruction to the return of blood from the brain, both by the contraction of the muscles of the

neck, and the suspension of respiration during the paroxysm. It differs from the coma of Bright's disease, in being secondary to the paroxysm, and not an initial symptom as in this renal disease.

Chemistry has shown two changes in the blood: A diminution of urea, and a diminution in the per cent. of albumen from 70 to 60, and even still lower, in the 1000. The changes in the urine are not less important than those in the blood. It contains a greater or less quantity of albumen. The attention of the physician being called to the existence of some of the premonitory symptoms, anasarca or some of the nervous symptoms, the urine is examined for a few days in succession, and is found to contain albumen, varying somewhat from day to day in quantity. The amount may be ascertained approximately by trial in the ordinary test tube, and observing the proportion that deposits at the bottom on standing. By such examination previous to, during, and after the attack, we ascertain the following facts: 1. The proportion of albumen varies during the premonitory symptoms, from day to day and may even disappear. 2. That its quality increases greatly during the attack under the influence of the renal congestion which accompanies it. 3. That it diminishes quite rapidly after delivery, when the disease is to have a favorable termination. 4. That the continuance of the albumen, in any considerable quantity beyond two or three weeks after the attack, denotes a more serious change in the kidneys, and adds greatly to the gravity of the prognosis.—*Bailly.*

According to Braun, the urea is always diminished in its proportion to the 1000 parts, and sometimes it is altogether absent. The uric acid is also generally in small quantity. The other constituents of the urine vary considerably. The uroxanthine is in increased quantity. The quantity of urine is always much diminished, and in some cases may even extend to suppression, and the suppression, partial or complete, may continue for several days. It is generally of a light amber color, and usually remains clear on cooling, but not always; an abundant precipitate of the earthy salts may form and give it a milky appearance, which disappears on heating. The light-colored hazy specimens of urine usually contain the largest amount of albumen.

The specific gravity varies from 1010 to 1030. In the deposit formed in the urine, in the first 24 hours after evacuation, besides the mucous and blood globules and epithelial cells of the ureters, some fibrinous casts of the tubes of the kidneys may be found. If, however, the urine is alkaline they will be dissolved by the bicarbonate of ammonia developed by decomposition. They proceed from the uriniferous tubules and are the result of coagulation of the exuded albumino-fibrous exudate, and mark the transition from the first to the second degree of albuminous nephritis.

The *duration* of the disease is *brief*, the *course* is rapid, the termination is one of three: Recovery, death, or the development of some other disease resulting from the convulsions. The most frequent result is recovery, either quite prompt and complete, or more or less protracted, dependent upon the degree of renal lesion, and the cerebral injury. In the more fortunate cases the casts disappear from the urine in two or three days, the albumen in from six to ten days after delivery, and the œdema is removed in about a week.

Death is a very frequent termination of this terrible disease. Great diminution of the urine with an amount of albumen sufficient to cause coagulation of almost the entire amount of urine, under the action of nitric acid, violent cerebral symptoms following paroxysms at short intervals, form a totality of symptoms sufficient to strongly indicate a fatal termination.

Death may occur during the tonic spasm when that continues more than half a minute. But this is exceptional. It usually occurs during the stage of coma. It seems then to result from the nervous disturbance, and from the derangement of the oxygenation of the blood produced by the repetition of the paroxysms. Nothing can rescue the patient from the profound coma into which she is plunged, the pulse is increased in rapidity, the respiration becomes more and more embarrassed, and a low progressive asphyxia puts an end to life.

It is both asserted and denied that violent and irregular contractions of the uterus, caused by the convulsions, may occur.

Hæmorrhage into the brain may occur, beyond question. It, and the effusion into the brain are the result of the afflux of blood

to the brain during the convulsion, and is characterized by paralysis, a symptom which is absent in uræmic cerebral disease, not complicated with cerebral hæmorrhage. Of course apoplexy cannot be recognized until the general muscular relaxation has subsided in order to ascertain the inequality of the action of the muscles of the two sides. It has occurred that the general relaxation of the muscles and the deep coma have so masked the paralytic condition that the cerebral hæmorrhage was only discovered at the autopsy. This same sanguineous effusion may give rise to one of the complications of eclampsia, to-wit, meningitis, from which the patient may at a later date succumb.

Other accidents may follow eclampsia, viz.: Congestion and apoplexy of the lung; violent concussion of the abdominal viscera, and the painful manipulations may produce inflammations from which the patient may die after having escaped the immediate dangers of the convulsions.

It is now pretty generally believed that an albuminuric patient is more likely to suffer from uterine hæmorrhage.—*Blot.*

According to the researches of Imbert-Gourbeyre, chronic Bright's disease follows in about one case in ten.

Only in a treatise on the subject of eclampsia would it seem necessary to speak of the diagnosis. The cases are usually so well marked as scarcely to admit of error on this point.

The mortality varies from one in two to one in three, according to different authors. In a large number (119) collected by Bailly, the deaths were 51 in the 119 cases—42.85 per cent. The proportion of deaths will depend upon the degree of one or more of the following symptoms: The degree of albuminuria, the violence of the nervous attacks and the rapidity of their succession, and the depth of the coma and insensibility. The period at which the attack comes on will have an influence on the prognosis. Aside from the paralysis or paresis which follows the cerebral effusions, the cases generally recover from the other functional effects of the disease. The disturbance of vision and partial amaurosis quite frequently continue for some weeks, but in the end generally disappear. While labor is precipitated by the convulsions, their repetition is also excited by the pains of labor.

Upon the general acceptance of the above view the profession is in accord as to the advisability of a speedy delivery at as early a date as the maternal condition will admit. The following facts from Braun will make this point clear: After the delivery of the child and the removal of the after-birth, the attacks suddenly ceased in 37 cases out of a hundred, they became weaker in 31 cases, and they continued with the same severity in only 32 cases in the hundred.

Eclampsia, while a very dangerous disease for the mother, is still more fatal to the child. In the majority of the cases the death of the fœtus is anterior to its birth, and results mainly from the disturbance of the uterine circulation arising from the nervous paroxysm. The fœtus may be dead for a number of days before the attack, and the death may be the result of the deteriorated condition of the mother's blood, or the manipulations in delivery may cause the death of the child; or again, it may die within a few days after delivery from debility.

Pathological Anatomy. — Investigations of the structural changes of the nerve centers, as connected with eclamptic convulsions, have been thus far rather negative in their results. These anatomo-pathological changes are either so transient or so slight as to evade the investigator. In many cases, dead of eclampsia, no trace of structural change can be found in the brain or in the envelopes. In others there are modifications recognizable, but they are scarcely of such a nature and degree as to account for the intensity of the effect—the convulsion. In fact, aside from the effects common to other parts of the body, viz., serous effusion, the more marked modifications are the result of the convulsions. and follow them rather than being the cause of them. I have previously spoken of the effusion, and occasionally of a hæmorrhagic lesion within the cranium. This frequent absence of any organic lesion of sufficient significance to account for the results, has caused the investigation to be extended to the fluids of the body, and to attribute the functional disturbances which form the symptomatology to changes in the blood. Theory and observation seem both to sanction such a view.

"The only constant organic changes in the eclamptic woman are those of which the kidneys are the seat. If they have es-

caped a certain number of observers it must be attributed to the want of proper means used by them in the investigation. Indeed it is alleged, since the works of Frerichs (1851), Bach and Imbert-Gourbeyre (1856), that in the kidneys of all subjects dying from paroxysms of eclampsia, the anatomical alterations which characterize the different degrees of Bright's disease, are observed. The existence of these lesions cannot be the subject of any doubt. The naked eye is not ordinarily sufficient to ascertain them, but the microscope reveals them where the unaided vision is unable to discover them. * * The alterations of the kidneys, I repeat, are never absent to those who know how to seek them with proper means, and constitute, properly speaking, with the physico-chemical alterations of the blood and urine, the pathological anatomy of eclampsia."—*Bailly.*

The only general condition which seems to have a positive value in predisposing to eclampsia is the puerperal state. The primiparous are far more liable to the disease than the multiparous, since four out of five cases of eclampsia are of the former class. In my own observations they have all been of the former class, though that was a mere coincidence. The principal predisposing cause is connected with the structural and functional condition of the kidney. Without arguing the case, or giving the authorities in favor of the view that albumen is to be found in all cases of eclampsia, though it may not be found at every examination, I assume that it may be found at various times; and the opposite condition is of so rare occurrence as to have no effect upon the fact of its existence in all cases when carefully sought for at periods sufficiently frequent. Blot, in examining 205 women, who came into the lying-in ward of the Maternity Hospital, found albuminuria, more or less pronounced, in 41—that is, one in five—all supposed to be under the influence of labor. The proportionate number albuminuric, taking the states of pregnancy, labor and lying-in, is not definitely known from exact research. The close relationship which exists between eclampsia and puerperal albuminuria, whether as to cause and effect, or of community of origin, adds to the importance of the study of the origin of either, for it may be that the discovery of the cause of one will be to make known the cause of the other.

The questions, as Bailly puts them are: Is the albuminuria of the puerperal state the result of an affection foreign to the kidneys, or is it connected with a pathological condition of these organs? And in the latter supposition, does it depend upon a simple functional disturbance of the gland; or, on the contrary, is it linked to a constant and primitive lesion of this same gland—albuminuous nephritis, acute Bright's disease; or again, is it due, as the case may be, to the one or the other of these mechanisms, or successively to both, in the same woman. These different views have been supported, and are to-day held, by men of great attainment. Again, another view is that the urinary disturbance being at the beginning only functional, in time produces an organic alteration, which latter in its turn produces albuminuria. It has been still further argued that at one time it may depend upon a functional disturbance, at another an organic. I am disposed to believe in a definite condition, especially as to the cause of albuminuria (leucomuria) of the later months of pregnancy.

1. The theory of the supra-albuminous condition of the blood (of the solid portions not of the serum), while being interesting in some of its features, is far from being able to explain many of the accompanying facts.

2. Puerperal albuminuria is caused by the serous polyæmia, which the puerperality produces. This position was held by some of our prominent writers on obstetrics, among whom we find Maugenest, an author of repute (1867). This is rather supposed than proven in its assumed facts.

3. Puerperal albuminuria is produced by a temporary, or by a permanent disease of the kidneys. To this proposition the majority of authors hold; and the number is daily increasing. And the works of Bach, Bailly, Litzmann, Frerichs and Braun no longer permit of doubt that the albumen in the urine of the pregnant woman has its origin in the pathological condition of the kidney. Microscopic examinations of the kidneys of subjects dead of eclampsia, show beyond question the nature of the morbid change, viz., acute Bright's disease or albuminous nephritis. Imbert-Gourbeyre was the first to insist upon this constant relation or the co-existence of acute Bright's disease

with eclampsia. Bailly is especially emphatic on this point, as expressed in the following sentence: "Therefore, for us at least, without doubt * * we admit that albuminuria, connected with eclampsia, finds its cause exclusively in an acute Bright's disease."*

The modus operandi by which the compression and restraint upon the renal circulation is produced has been so clearly set forth by Braun, as to justify our quoting it here: "The first stage of acute Bright's disease consists in a hyperæmia caused by the congestion of the venous blood, affecting one or both of the kidneys at once. Upon this state follows, immediately, an extravasation of fibrinous exudation into the malpighian corpuscles, which invades to a certain distance the interstitial tissue, envelops the vascular net-work, escapes partly into the urinary canaliculi and often to be secreted in a fluid form with the urine. However, most commonly the albumen of the exudation passes alone with the urine, while nearly all the fibrinous matter coagulates in the tubuli and in the cortical substance. The latter remains there for a longer or shorter time, till it is expelled at the same time that the exfoliated epithelium is evacuated with the urine in the form of the tubes of Bellini and of Ferrine. The nutrition of the uriniferous tubes at last suffer from the inflammatory condition and from the production of an abnormal plasma around their epithelial cells; the tubuli are transformed by a retrograde metamorphosis into cells filled with a detritus rich in fat; a similar change takes place at the same time in the fibrinous matter, which has remained there long. The walls of the tubuli, deprived of their epithelium, actually destroyed, come in contact, and the sojourn of the false membrane gives rise to a fibrous cicatricial tissue, amorphous, which displaces the glandular parenchyma and causes the depressions which we observe on the surface of the kidneys; we observe at the same time, upon the external surface or in a section of the cortical substance, other tubuli still filled with fat. The more the destruction of the tubuli has advanced the more of course is the volume of the

* Loc. cit., p. 320.

kidneys reduced, since at the same time a part of the circulatory apparatus of nutrition is also destroyed by obliteration."

While this may very clearly explain the cause and condition of things in the later months of pregnancy, it does not seem an adequate explanation for the leucomuria of early pregnancy; and Braun also says that future observations must decide whether eclampsia of the early months of pregnancy is or is not co-existent with Bright's disease. The two do not necessarily stand to each other as cause and effect; but they are both rather as symptoms of a common cause, and not co-existing by mere accident. The proximate or determining cause of eclampsia is very difficult to determine positively. Symptoms which are so uniform in character should have some definite cause or constant condition to produce them. And it is this definite cause which we should strive to discover. Various theories have been put forth, some of which refer the determining cause to material changes in the brain or spine; some to reflex irritation; some to anæmia of the brain, and lastly others to the composition of the blood as not being appropriate for the healthy nutrition of the nervous system (brain and spine). The latter view has the greatest number of adherents. If this be the true theory, so far as it goes, then what is this special deviation from the normal condition? Uræmia had its day and was succeeded by ammonæmia—Frerichs holding that the change of urea to carbonate of ammonia took place in the blood, and Treitz that the change took place in the intestine and was absorbed into the blood, hence the term ammonæmia as applied by Jaccoud. Still another theory of the pathogeny of eclampsia is, that it depends upon other retained materials of denutrition, viz., *extractive matters* of the blood. It is claimed by Bailly that this explanation has the greatest number of scientific supporters, because it rests upon an undeniable fact. The exact nature of the blood poison is not yet fully demonstrated, at least not to the satisfaction of chemists, and we must await further developments at this particular point.

TREATMENT.

The treatment of eclampsia naturally divides itself into preventive and curative. The preventive treatment, made use of

during pregnancy, will be chosen and applied according to the existing symptoms. Blood-letting will seldom be necessary; it may be employed in cases giving evidence of local congestions and disturbance of the brain when found in plethoric, full-blooded individuals. My own practice has been to keep the emunctories active, especially giving attention to the action of the bowels by the administration of aperients or cathartics, as may be indicated; and to the kidneys by the use of alkaline diuretics, in combination with digitalis.

During labor it is important to diminish its *severity* by various anodyne medicines; and to abbreviate its *duration* as much as practicable, in order to lessen as far as possible the exciting causes of the convulsion. If the individual have an abundance of blood, venesection, at a time when the symptoms are such as to indicate the imminence of an attack of convulsions, would be proper. More frequently, however, the quieting remedies, of which morphine and chloral stand preëminent, would be preferable. Morphine administered hypodermically has been in use in this disease for many years. I saw it used more than twelve years ago in the wards of Prof. Braun in the Vienna Hospital, both when the attack was threatening and after it had occurred. Chloral, in doses sufficient to render the patient in decided measure insensible to pain or excitement, is a remedy until quite recently much relied on, and is at present held in high favor by some.

It may be eminently proper to terminate the labor by artificial means under the same general conditions as would be advisable after one or more convulsions has occurred, of which I shall speak later.

The curative treatment is *medical* or *surgical*.

After a convulsion has occurred, or even when one has announced its near approach by the peculiar symptoms imme diately preceding it, there are two principal therapeutic measures to be considered, viz., venesection and narcotics; and one or both of them should be brought into requisition at once. If the patient is moderately full blooded, bleed moderately; if plethoric, bleed freely. Follow this immediately with a full dose of morphine hypodermically, or with chloral in forty or sixty-grain dose, given *per os* or *per anum*, as circumstances may indicate.

The question of blood-letting has been much discussed, many favoring it—regarding it above any other measure. It has received the sanction of a greater number of the profession than any other remedy. At the same time we may not fail to record the fact that many able men in the profession prefer to depend upon other means. This is not the time to enter into the full discussion of the reasons assigned on the one hand in favor of, and on the other against, the practice. The amount of cerebral congestion found *post mortem* in many cases, would seem to indicate not merely the propriety but the positive demand for this practice, ordinarily so promptly relieving intercranial congestion. From ten to twenty ounces taken in a bold stream will give efficiency to the other remedies. This measure is as old as the history of the disease.

The next remedy in order is *morphine;* and to get the effect as rapidly and as decidedly as possible it should be given hypodermically. This remedy, and this method of using it, is highly endorsed and has been followed by remarkably favorable results, especially so when used in heroic doses. It is gaining in favor with the profession. In the July (1880), number of the *American Journal of Obstetrics*, Dr. C. C. P. Clark gave a very enthusiastic report of the use of large (two-grain) doses of morphine hypodermically administered. In choice and vigorous language he speaks quite disparagingly of our pathological knowledge of the disease and of the general treatment also, except of the heroic doses of morphine. But we cannot yet adopt his over sanguine expression, "When obstetricians shall fearlessly follow out the rules that I laid down, this fell disease will cease to be a terror."* He cites several cases successfully treated by his heroic plan; and Dr. C. P. Faust, of Graham's, S. C., follows in the same train.

Chloral finds very zealous advocates in recent years among distinguished members of the profession. In a recent article by Dr. H. H. Kane (*American Journal of Obstetrics*, April, 1881), on "Chloral Hydrate, its Uses in Obstetrical Practice," the following language may be found: "It has, however, cured more

* American Journal of Obstetrics, April, 1881, p. 416.

cases than any single remedy or combination of remedies known to us." If we should take the assertions of Drs. Clark and Kane, above quoted, we might conclude that puerperal eclampsia had become, under these forms of treatment, shorn of its terrors and of most of its former fatality. We do not, however, share in such enthusiasm. Medical statistics, to be of value, must include large numbers, and the influencing circumstances must be held strictly to account. Dr. Wm. Goodell, as reported in the Proceedings of the Philadelphia Obstetrical Society (*American Journal of Obstetrics*, April, 1880), says he has never lost a case; and that he was one of the first in the city to use chloral in eclampsia. He also spoke highly of the hypodermic injection of morphia after venesection. The best effects seem to be obtained when administered as an enema sufficiently diluted with syrup or beaten egg, and introduced well up into the intestine; the initial dose should be from forty to sixty grains, according to the urgency of the case. *Chloroform* is a kindred remedy of much repute also. Some years ago Braun reported sixteen cases thus treated, all resulting in recovery. Very few, if any others, have been equally successful, and many able authors have rejected it. As in any other case, it should be given with caution, but may be continued for many hours. Our own preference would be to use it in conjunction with other remedies.

There are other remedies, of less efficiency, which need only be mentioned here. In case of the urine being albuminous, act upon the kidneys with infusion of digitalis and acetate of potash. A free cathartic may be given later—compound jalap powder or ol. tiglii.

The surgical management is of great importance to abbreviate the duration of labor, and to mitigate or terminate the irritation and suffering. Delivery should be effected as soon as practicable. If the os is dilated or dilatable, Paul Dubois says, "evacuate the uterus just as soon as it can be done without violence." The wisdom of this advice applies to both mother and child. Braun reports the fact that of one hundred cases of eclampsia the convulsions ceased promptly after the expulsion of the fœtus and after-birth, in fifty-seven (57) cases; grew weaker in thirty-one

(31), and continued with the same violence in only thirty-two (32). If labor has not commenced and the convulsions do not yield to the usual remedies—morphine, chloral and chloroform, I should advise delivery by artificial aid, especially so if the patient is albuminuric. In this case the os must be dilated by means of the hand or other approved means, and delivery effected by the best means which the circumstances require.

The three following cases will show the treatment, which in my judgment is suited to the disease, or threatened disease, in the three periods—*before*, *during* and *after* labor:

I. Mrs. H., aged thirty, primipara. Considerably œdematous in both upper and lower extremities. Urine albuminous to the extent of one-third. No casts found. Suffered at times considerably with headache. These conditions existed for some weeks before confinement. Gave pretty freely of comp. jalap powder; also infusion of digitalis and acetate of potash. Labor came on at the expected time; and was moderately easy. Patient was considerably nervous; but there was no serious complication, and the recovery was rapid and complete.

II. Mrs. S., aged twenty-six, primipara. Labor at full term. (There had been no symptoms such as to attract attention previous to labor, which was somewhat tedious). Complained at one time of headache. First convulsion came on before the os was dilated or dilatable. In half an hour a second paroxysm came on. The os being now dilatable, delivery was promptly effected with the forceps. The child was still-born, but was resuscitated after considerable effort. Morphine was given after the first convulsion, by the mouth. Two convulsions occurred after delivery. Bled after the third to about eight ounces. Gave infusion of digitalis and acetate of potash. Urine after delivery was loaded with albumen and casts. Consciousness did not return for about thirty hours. The diuretic acted well. Mother and child recovered. The former had an attack of neuralgic rheumatism four months previously, threatening abortion. Has been delivered safely twice since, the last time followed by a severe attack of inflammation in the uterine region, and has had attacks of rheumatism since getting up. On November 16

(date of last confinement), when I first saw her, the os was fully dilated; membranes intact; pains short, not expulsive to any considerable degree; much pain in the right inguinal region, and frequently extending down the limb. A short time before the delivery of the head there was mental aberration for the period of half a minute or more. She said her head ached, and she asked, "Go where?" But it soon passed off. The delivery was effected without special difficulty. On the previous day the urine showed a decided trace of albumen, and a specific gravity of 1010.

III. Mrs. W., aged 43, primipara. Saw her at one o'clock, A.M., in consultation. Head of child resting on perineum. Body of patient œdematous, lower extremities very much so. An hour or more previously she had complained of a severe pain in her head and dimness of vision. About twenty minutes previous to the time of my seeing her she had a convulsion, and was now entirely insensible. She had taken some chloral. Proceeded to deliver with forceps; but during the preparation she had a second severe convulsion, immediately after which delivery was effected. Gave chloral by the mouth, and morphine hypodermically. One (or two?) convulsions occurred after delivery. The coma being very profound I bled her eight or ten ounces, there having been a moderate loss otherwise. No more convulsions. Examined the urine drawn after delivery, and found it three-fourths albumen, and a later examination showed an abundance of casts, under the microscope. No examination had been made, strange to say, for about two weeks. Gave digitalis infusion and acetate of potash. Consciousness began to return in about twelve hours, then gave a free cathartic of comp. jalap powder, which acted well. Partial paralysis of tongue and side of face continued for several weeks, double vision, showing in the beginning decided intercranial lesion. Child recovered without difficulty, and the mother finally, entirely, I believe.

I have observed other cases, but these will serve to illustrate in part the plan of treatment in the three stages of pregnancy. The following table from Bailly, modified to suit my views of treat-

ment, will more fully set forth the management to be adopted under the varied conditions:

CONFIRMED ECLAMPSIA. CURATIVE TREATMENT.	1. Os completely closed. (Pregnancy)	A. Medical means.	Bleeding from the arm to the extent of 15 to 30 ounces at most; leeches to the mastoid processes. Calomel and Jalap. [I would give instead the Comp. Jalap powder.—G.] Cold applications to the head. Chloroform. [I would would say chloral and morphine, one or both.—G.]
		B. Surgical means.	If there is postponement of spontaneous labor, notwithstanding the cerebral disturbance, produce it artificially.
	2. Os incompletely dilated. (Labor.)		General rule: Wait to extract the child till the dilatation is spontaneously completed. Very exceptionally and only when the frequent and grave paroxysms resist medical treatment, complete the dilatation of the orifice with the hand or make incisions around the os for the purpose of terminating labor more promptly.
	3. Os dilated or dilatable.		If the contractions are strong, the child small, the labor rapid, and the paroxysms seldom, trust the delivery to nature; in the opposite conditions terminate it by turning, or with the forceps.
	4. Labor terminated.		Remove the placenta and clots without delay, then employ medical means.
ALBUMINURIA, ECLAMPSIA IMMINENT OR ONLY POSSIBLE. PREVENTIVE TREATMENT.	1. Pregnancy.	A. Medical means.	Moderate bleeding, [?] purgatives, sudorifics, baths, frictions, exercise, tonics, chalybeates.
		B. Surgical means.	Produce delivery (Tarnier) under certain conditions above mentioned. (Theoretical precept not yet settled by the facts.)
	2. Labor.		Trust it entirely to nature if its course is regular and rapid. Under opposite conditions terminate it by turning or by means of the forceps when the dilatation of the os will permit.

ARTICLE II.

A BRIEF REPORT OF ONE HUNDRED CASES OF RÖTHELN. By ROSWELL PARK, A.M., M.D., Chicago. (Read before the Illinois State Medical Society, May 17, 1881.)

It is not my purpose to treat in a general way of rötheln, but simply to speak of the peculiar features of an outbreak of the disorder in the Protestant Orphan Asylum of this city, of which I have professional charge. The disease had prevailed to a small extent in scattered localities through the city, and had been recognized as such, about a month before its appearance in the above institution. The first of my cases was diagnosed about the 8th of April, and during the five weeks following, ninety-five

children out of one hundred and forty, and two adults, manifested the usual symptoms of this exanthem. These, with a dozen or more cases in private practice, swell the number under my observation to more than a hundred.

Many circumstances rendered it difficult to make out a definite period of incubation, because I could not trace the contagion with sufficient reliability. One feature is worthy of attention first of all, and that is, that only two-thirds of the children exposed were attacked. This is analogous to what we see in the case of the other exanthems, scarlatina for instance; of three children exposed only two may take the disease.

Another point worth mentioning is, that most of these children had already had measles the previous winter or spring: for, during the early months of 1880, eighty-eight children in the same institution were attacked with morbilli.

There were no definite premonitory symptoms. Occasionally the patient would be languid or fretful for a few hours before the eruptive outbreak, and a very few coughed a little, but not in any way to attract particular attention. Along with the appearance of the eruption about twenty-five per cent. presented symptoms like those of the outset of measles. In ten per cent. of the whole number the bronchial irritation, conjunctival suffusion and general appearance were so characteristic that if these same children had not had measles to my certain knowledge, I should have been tempted to diagnose them as such.

The papillary eruption needs no description. I may say, however, that in three or four cases there was a slight tendency for it to become confluent. It appeared first on the neck, throat or behind the ears, traveling downwards, and was usually very distinct on the forearm and wrist. With the appearance of the first papules there was, in almost every case, so much flushing of the cheeks that any manifestation there was hidden from view. The same papules were also plainly visible in the roof of the mouth even before they were distinct on the surface.

Twenty per cent. of the cases had pharyngitis or tonsillitis, but in mild form. Fully one half of them had marked adenopathy in the cervical region or under the tongue.

While the pulse rate was rather high in some of my little

patients, the temperature was never any higher than seemed reasonable under the circumstances. Nausea and vomiting were exceedingly rare.

Not one of them suffered from any complication of importance during the progress of the rötheln. But undoubtedly they were left quite susceptible to dangerous sequelæ. Four were prostrated with pneumonia in severe form, though all recovered; and several others suffered from more or less severe bronchitis or croup. This experience has taught me that there is necessity for no small amount of caution for some time after convalescence. In several cases I noticed a very slight furfuraceous desquamation.

The progress of the disorder was, in all cases, toward recovery, and with little or no treatment. I only advised medication when sympathetic fever ran high, when bronchial irritation was marked, or when the bowels were sluggish.

These cases are, I believe, fair samples of the disease which has visited our city, in an epidemic form, this past spring; and while individual cases have hitherto hardly been rare enough to be considered curiosities, yet the disease in its epidemic features has been new to the younger portion of the profession here. For this reason I am perhaps justified in reporting these cases without further apology.

1558 Wabash Ave., May 15, 1881.

ARTICLE III.

DIFFUSE IDIOPATHIC ANEURISM OF THE ANTERIOR TIBIAL ARTERY. TETANUS FOLLOWING AMPUTATION OF THIGH.

By G. FRANK LYDSTON, M.D., late House Surgeon Charity Hospital; Resident Surgeon, State Emigrant Refuge and Hospital, Ward's Island, New York.

The patient, T. R., a painter by occupation, and thirty years of age, entered Charity Hospital, September 4, 1880. He gave a history of having had malaria and rheumatism, and several attacks of gonorrhœa, but said that he had been otherwise

healthy until four years since, when he contracted several sores upon his penis, which were termed "mixed sores," by his physician. In about eight weeks thereafter, he developed secondary syphilis, which was treated by a mercurial course with apparent success. Some time afterward however, he developed several tertiary ulcers upon his legs, for which he was treated in the hospital, and which healed readily in a few weeks. About eight weeks previous to last admission, several ulcers appeared upon the outer aspect of the left leg, just below the knee over the heads of the tibia and fibula. These were evidently syphilitic in character, and he was accordingly placed upon the "mixed treatment," with a large excess of the iodide of potassium. The benefit from this was only temporary, the ulcers soon beginning to extend, and the bone becoming involved. The odor from the ulcers soon became so offensive that it became necessary to transfer the case to the surgical pavilion. During December, six arterial hæmorrhages occurred from the central ulcer over the head of the tibia, which on several occasions were difficult to control. Necrosed bone was now plainly visible upon the heads of both tibia and fibula at the bottom of the ulcer. On December 10, a consultation was called. In the situation of the ulcer, there was now a large elastic tumor, and the finger being introduced into the ulcer, portions of blood-clot were detached, which seemed to be a portion of the tumor. Exploration decided that the tumor contained blood. During the examination, and while the limb was not under manipulation, severe arterial hæmorrhages occurred, rendering the application of a tourniquet necessary. The diagnosis of "diffuse aneurism" was made, and as the condition of the parts rendered deligation of the femoral unpromising, amputation of the thigh, in the lower third, was determined upon. On December 14 the operation was performed; Esmarch's "bloodless method," and the Lister spray and dressing being used. The amputation was made with lateral skin flaps, the femur being divided at about the middle of its lower third. The patient reacted from the anæsthetic very slowly, and the pulse being very feeble ℥ss of brandy was given. The subsequent course of the case was as follows:

December 15. Temperature 103° F.; pulse 128. Patient feeling comfortable.

December 16. Temperature 101°; pulse 100. Dressings removed and stump redressed under the carbolic spray. Stump looking well.

December 17. Temperature 101.4°; pulse 116. Patient feeling well.

December 18. Temperature 101.5°; pulse 110. Stump redressed and all stitches removed excepting two at upper angle of wound, on account of sloughing of the flaps to some extent.

December 19. Temperature 100.5°; pulse 112.

December 20. Temperature 100°; pulse 104. Stump redressed and dressings ordered to be renewed three times daily. Stump looks fair, but considerable suppuration going on.

December 21. Temperature 100°; pulse 104. Patient quite comfortable.

December 22. Patient complained of a sense of depression, and expressed the fear "that something was going to happen to him," but he had no definite symptoms. Temperature 100°; pulse 100.

December 23. Patient complained of "tingling and twitching sensations in the stump, and of some stiffness and soreness of jaws. Countenance anxious and pinched. Temperature 100°; pulse 110. Was ordered sol. Magendie min. x, t. d.

December 24. Patient much worse. Trismus still more marked than on day previous. Some involuntary spasms of the extremities, notably of the affected thigh, have occurred since last visit. Countenance more pinched, and extremely sallow. Temperature 100°; pulse 112. General appearance suggests a certain degree of septicæmia.

December 25. Several slight convulsions of limbs occurred during the night of the 24th. Jaws cannot now be opened sufficiently for the introduction of food. Fluids are expelled when drunk, and return partially through nostrils, suggesting paresis of soft palate. Any attempt to force open the jaws caused a slight general convulsion without loss of consciousness, and of short duration. Patient very feeble, and evidently sinking rapidly. Rectal alimentation and stimulation were resorted to, but to no purpose. Chloral hydrate was also administered per enema. Temperature 104°; pulse 125. Death occurred on this date, 25th, at 11 P. M.

No severe muscular spasms occurred at any time, and there was no opisthotonos. The tetanoid symptoms were, I think, modified in great measure, by the septicæmic element which evidently existed. No autopsy was held, but an examination of the diseased limb was made shortly after its removal.

It was found that an ulceration and solution of continuity of the anterior tibial artery, about half an inch from its origin, had occurred. The eroded ends of the artery were found free, and connecting with a cavity about as large as a good sized orange, which was filled with disorganized blood-clot, occupying the inter-osseous space at the heads of the bones, the space between which was greatly enlarged from necrosis. This cavity had been formed by the pressure of its contained blood, the muscles having been displaced in its formation, and a pseudo sac formed by plastic deposit in the surrounding tissues. The degeneration of the artery was of considerable extent, there being some distance between the distal and proximal extremities in this situation, with portions of the degenerated arterial coats, present in the contents of the sac.

ARTICLE IV.

THE PATHOLOGY AND TREATMENT OF YELLOW FEVER; WITH SOME REMARKS UPON THE NATURE OF ITS CAUSE AND ITS PREVENTION. By H. D. Schmidt, M.D., New Orleans, La. (Continued from page 70, July No., 1881.)

GENERAL PATHOLOGY.

We have now studied the clinical phenomena of yellow fever, together with the pathological changes taking place in various tissues and organs during the course of the disease, and revealed by a post mortem macroscopical and microscopical examination; a comparison of these two sets of phenomena with each other, for the purpose of determining, as far as possible, the relationship necessarily existing between them must, therefore, be the next object in view. Formerly, when diseases were classified by certain groups of clinical phenomena, or symptoms, which they presented, it was chiefly these symptoms to which the physician directed his

attention, and by which he was guided in his treatment, consisting for the most part of remedies to which, in the cure of each particular disease, a specific action was attributed. With the rapid development of pathological science, however, the custom of treating disease in this empirical way was gradually abandoned for more rational methods, based upon a thorough knowledge of the intrinsic nature of the disease, obtained from the nature of the pathological changes observed after death in the tissues and organs. But even a knowledge of these changes, wrought upon the internal organs by the morbid process of one or the other disease, is insufficient to safely guide the physician in his efforts to obtain a cure, unless he is able to tell from the symptoms, presented by the particular condition of his patient, the exact and simultaneous condition of these organs; in other words, to form an accurate scientific diagnosis, the highest accomplishment of the practicing physician. In a considerable number of diseases, especially those of the heart and respiratory organs, medical science has reached this aim, and I am confident that, to a considerable extent, the same may also be accomplished in yellow fever.

In yellow fever, as in other acute infectious diseases, the most prominent phenomenon observed is that of fever; I shall, therefore, introduce my remarks on the general pathology of this disease with a brief consideration of the observed facts, relating to the *febrile process in general.*

Various theories on the phenomenon of fever have been advanced by medical men at all periods of medical history. The older theories were all based upon the principles of "humoral pathology," according to which not only fevers, but all other diseases depended upon a contamination of the blood, caused by the reception of noxious substances from without, or, in consequence of certain changes going on within the body, the febrile process being an effort of nature to expel the noxious matter. It was *Hoffmann*,* who first directed the attention from the fluids to the solid tissues, as the seat of the disease. He sought the source of fever in the nervous system, believing that it was orig-

* *G. B. Wood*, "Practice of Medicine," fourth edition, Vol. I, p. 101.

inally depending upon a spasm of the capillaries, and considering the heat of the skin and the arterial excitement which follow as the mere reaction of the system, necessary to overcome this spasm. A very similar theory was entertained and promulgated by *Cullen*, and, also, in a more or less modified form, by some other authors. Another theory of fever was, in the beginning of this century, put forth by *Clutterbuck*,* who regarded all forms as of local origin, and depending upon local inflammation. The so-called idiopathic fevers, therefore, he regarded as inflammation of the brain, designating them "encephalitis." *Broussais*, in announcing his doctrine of fever, agreed with Clutterbuck in denying the existence of essential fevers, but differed from him in ascribing all those forms, previously denominated idiopathic, to inflammation of the mucous membrane of the stomach, or that conjointly of the stomach and bowels; in other words, being *gastritis* or *gastro-enteritis*. The impression which Broussais's theory had made, gradually faded, and there has been no doctrine of fever since which had a general prevalence, though the above theories of fever have, with various modifications, extended into our time. Thus, the neuro-pathological theory of Hoffmann and Cullen was, in a modified form, subsequently more developed by *Virchow*,† who regarded the phenomena of the febrile process, especially the elevation of temperature, as depending upon a depression (Nachlass) of certain parts of the nervous system, which he supposed to act as moderators of the production of heat. In still more recent times, however, the old theories based upon humoral pathology, appear to recover their lost grounds.

Although the most prominent symptom of fever, the abnormally increased heat of the body, has always been attributed to an increased waste of matter derived from the tissues, the discovery of a correspondingly increased formation of urea, found in the urine during the febrile process, and verifying this supposition, belongs to more recent times. The precise nature of the febrile process itself, and the true cause of the increase of bodily temperature, however, remained still unknown, and gave rise to

* L. c., page 103.

† *R. Virchow.*—Handbuch der Pathologie u. Therapie, 1854, Vol. I, p. 33.

very numerous and laborious investigations, made by a considerable number of investigators, such as *Jochmann*, *Traube*, *Moos*, *Redenbacher*, *Uhle*, *Ringer*, *Brattler*, *Wingl*, *Wachsmuth*, *Warnecke*, *Huppert*, *Tschsschichim*, *Breuer*, *Chroback*, *Leyden*, *Ranke*, *Senator*, *Naunyn*, *Unruh*, and others.* A most able treatise, covering the whole subject, and embracing his own labors as well as those of others, was written by *Senator*, who, for his extended contributions to the knowledge of the febrile process, as far as it goes, may be regarded as a prominent authority. And as some of his special views seem to have been corroborated by still more recent observations, I shall not hesitate to extract from his treatise in the following discussion.

In the above mentioned investigations, the attention was particularly directed to the existing proportion between the production and discharge of heat, carbonic acid and water; and the most prominent facts thus far elicited were: an increased formation of urea in the urine, and an augmentation in the discharge of carbonic acid, water and heat.

The increased formation of urea is not depending upon a greater activity of the normal exchanges of matter in general, but rather upon a greater waste of albumen. The quantity of urea excreted during the whole fever amounts in the average to more than twice the quantity excreted during the absence of fever, but otherwise under the same conditions; that is, when the body is kept on fever diet. The average quantity of urea excreted on such diet, and taken by Senator as a basis, was 18 grms. in twenty-four hours.

The increase in the quantity of urea secreted begins, as was first observed by Ringer, Traube, Joachmann, Uhle and Redenbacher, before an elevation of the bodily temperature is noticed. Senator, who corroborated this fact by his experiments on dogs, in which, contrary to man, the activity of the urinary function increases during the febrile process, enlarges upon this phenomenon as follows: "Whilst, namely, with the dog the quantity of

* *Virchow u. Hirsch*, Jahresberichte, etc., für die Jahre 1866–1872. Section: Allgemeine Pathologie.

† *Senator*.—"Untersuchungen ueber den Fieberhaften Process, und seine Behandlung.' Berlin, 1873.

urea discharged with the urine in about twenty-four hours may be looked upon as equal to that *formed* in the same time, such a presumption is not accepted as regards most febrile diseases in man. In the dog, at least in artificially produced pyæmia, the discharge of urea is favored by the augmentation of the water of the urine; and a loss of nitrogen by some other way than through the urine, as, for example, through the fæces, did not take place during the time of observation. With man it is different. Here, the amount of urine is usually diminished, and with this the chief source for the abstraction of nitrogen is limited. In consequence, the removal of the latter does not keep equal pace with the extensive disintegration of albumen; urea—or, as we may for the present suppose, bodies formed preliminary to urea—accumulate in the body, to be afterwards eliminated at a shorter or longer time after the defervescence of temperature, as the post-febrile augmentation of the discharge of the urea so frequently shows. Besides this, the last residue of oxydized nitrogen is certainly as yet not removed with the latter, but even in the succeeding days the origin of a portion of the discharged urea may still be referred to the time of fever. Moreover, the post-febrile augmentation of urea does not always show itself very conspicuously, as, quite frequently, the body rids itself of its surplus only very gradually, and an imperceptible transition from the febrile to the normal excretion of urea takes place, so that it is difficult to determine how far this is still depending upon the influence of the fever." He further remarks that in different febrile diseases, much nitrogenous material, in various other forms than the ordinary constituents of urine, leaves the body, or, at least, is abstracted from the exchanges of matter, not only with the stools, but with the expectoration, or in the form of albumen in the urine, as it frequently occurs with high temperatures. These losses, which affect the organism only during the fever, but not when in a state of inanition, also bear in favor of a febrile augmentation of the exchange of albumen; for, if these quantities of nitrogen were not abstracted from the exchange of matter, the organism would discharge still more urea during the fever. According to *Hoppe*, the quantity of ammonia in the urine, also, is considerably augmented in febrile diseases forming an expendi-

ture of nitrogen not taken as yet into account in determining the quantity of urea. If, therefore, in the fever of the dog, the augmentation of the excretion of urea may be regarded as in just proportion to that of the disintegration of albumen, this is not applicable in the same degree to man. On the contrary, the disintegration of albumen during the fever of man is augmented in a greater measure, as can be calculated from the increase of the urea, and must, therefore, amount to more than double the exchange taking place under the same conditions without fever.

The conditions for the discharge of carbonic acid are, according to Senator, during the hot stage of the fever better than normal; under the most favorable circumstances, the discharge during the day-time is augmented from thirty to forty per cent., while during the night, it is probably, as in the normal condition, generally less. The formation of carbonic acid during fever, therefore, can, in the most favorable case, only augment to below thirty to forty per cent.

From the above facts, Senator concludes that in the fever of man no uniform augmentation of the whole exchange of matter, viz., the albumen and fat, takes place. As regards the disintegration of the constituents of the body during the febrile process, however, there are other facts known, which not only confirm it, but moreover indicate, which particular tissues are affected by the augmented disintegration. Above all, it is the exact investigations of *Salkowski* upon the excretion of the alkaline salts, which have shown that the quantity of potassa excreted by a healthy individual on fever diet, is from three to four, or even seven, times augmented during the febrile process, while a similar augmentation of soda does not take place. The latter, on the contrary, appears to be excreted in a smaller quantity, which is perhaps owing to the existing antagonism in the excretion of potassa and soda, shown by *Boecker* and *Reinson*, and more recently by *Bunge*. It is furthermore known that in febrile diseases the coloring matter of the urine considerably augments in quantity, which, according to *Vogel*, amounts to even more than four times the quantity excreted by healthy individuals on an ordinary full diet. No final product is augmented in the same measure as the urea, coloring matter of the urine, and the potassa,

not even the phosphoric acid; it is even doubtful, after the careful determinations made by *Rosenstein*, whether its excretion—in the urine—is much augmented under the influence of fever. As regards the excretion of sulphuric acid, the greater part of which is likewise derived from the combustion of albumen during the fever, nothing certain is known, probably because its secretion does not regularly and rapidly follow upon its formation.

Those nitrogenous tissues which are rich in potassa and hæmoglobin, the mother substance of the greater part of urea, are therefore, especially prone to disintegration, namely: The colored blood corpuscles first of all, and then the muscles, while the brain and spinal marrow, with the nervous tissues in general, though likewise rich in potassa, may be excluded, as they furnish no coloring matter of the urine, and also because the discharge of phosphoric acid is not augmented. Experience teaches besides, that the central parts of the nervous system are least affected by the febrile consumption, or by that caused by inanition. The fact, that it is the colored blood corpuscles which, first of all, suffer and disintegrate during the febrile process, is corroborated by the observations of *Koerber*, which showed that the decomposability of the hæmoglobin was increased in febrile diseases, and the statement of *Mannasseïn*, concerning the general diminution of the colored blood corpuscles in size during fever and with an elevated temperature. But if those corpuscles and the hæmoglobin are destroyed to a great extent; if, in consequence, the body becomes poorer in those elements which effect the reception of the oxygen from the air and its conveyance to the tissues, and if other elements do not take charge of this work, then the activity of the processes of combustion in the body must necessarily decrease in the same measure for want of oxygen. The further statement of Mannasseïn, that blood corpuscles rich in oxygen, increase in size, as also when under the influence of certain anti-febrile remedies—quinine, etc.—is a further proof that the function of the colored blood corpuscles is lowered during the fever; that the latter appropriate less oxygen, rapidly decrease in size and disintegrate.

Accordingly, the organism cannot absorb as much oxygen, and in consequence cannot oxydize as much material of the body to

its final products during fever, as it can without fever and under the same nutritive conditions."

If, now, the one final product, urea, is found in too large a quantity, already consuming a certain quantity of oxygen more than usual, it follows that the other final products, which require still more oxygen, must naturally be diminished in quantity; and from this, it again follows that the combustion of the non-nitrogenous constituents of the body—the fat—and, accordingly, also the formation of carbonic acid during fever, cannot be simply augmented. The increased destruction of the colored blood corpuscles, the carriers of oxygen, then, forms another cause for the granular and fatty disintegration of the tissues, taking place here in a manner similar to the effect of poisons which act by abstracting the oxygen. The conclusion, therefore, is that in the majority of febrile diseases of man, the body decomposes exclusively, or, at least, more than normally, nitrogenous material, *i. e.*, albumen, and in consequence becomes relatively richer in non-nitrogenous, *i. e.*, fat. Without oxydation, only water, increased in quantity, can be formed from the ultimate products of the exchange of matter—by synthesis and dishydration.

The quantity of urine in fever is usually in proportion to the supply of liquids, but its secretion is more unfavorable than in the normal condition, as a comparatively smaller portion of the received water is discharged by the urine than in the absence of fever, but otherwise under the same conditions. The quantity of the evaporated water is augmented during the fever, and amounts to even a little more than that of the expired carbonic acid; therefore, the quantity of water lost by evaporation is comparatively large. In what proportion the whole loss of water stands to its formation during the fever—with the exception of extraordinary losses—can as yet not be determined. With a large supply of water some of it may, as in the normal condition, be retained. The variations in the weight of the body during fever chiefly depend upon these unstable relations between the supply and loss of water.

In comparing the economy or exchange of matter during the febrile process, as above stated, with that taking place in an individual kept on a perfectly normal diet, Senator remarks that the

only two substances of excretion, from which the exchange of of albumen and fat may be calculated, are urea and carbonic acid, and that the daily normal excretion of the former amounts from 25 to 30 grms., while that of the latter is from 700 to 800 grms. This, based upon the values of combustion as determined by *Frankland*—4,263 units of heat for 1 grm. albumen, and 9.1 units for 1 grm. fat—corresponds to an exchange of 74 to 93 grms. albumen and 193 to 240 grms. fat, with a formation of heat of 2072 to 2580 units. The fever patients upon whom the investigations relating to the excretion of urea and carbonic acid were made, rather belonged to those who in their ordinary normal condition show a low equivalent weight of nitrogen, and whose daily excretion of urea deviates perhaps little from 25 grms. If these individuals discharge, at least during the first days and with a high fever, 40 to 50 grms. in 24 hours, then this is, in the most favorable case, a surplus expenditure of 25 grms. at the utmost in comparison with their normal condition, or a surplus exchange of 74 grms. albumen at the highest rate. In supposing that this albumen were completely oxydized, as in the healthy condition, to form urea, $C\ O^2$ and $H\ O^2$, an augmentation of heat, equivalent to 315 units, would result, or more still, because during the fever more albumen is disintegrated than corresponds to the urea.

In corroboration of the statements and views of Senator, but more especially for their own intrinsic value, I will recite some of the more recent investigations regarding the exchange of matter during fever and inanition, obtained by *Zuelzer*.* They are as follows: "1. The total sum of the nitrogenous excretions is augmented during the state of febrile excitement, and diminished during the state of depression—inanition, convalescence. 2. The relative quantities of phosphoric and sulphuric acid—in proportion to the nitrogen—in the urine during the state of fever and hunger—do not exceed those found in muscle and brain. 3. In proportion to the nitrogen, more sulphuric acid is excreted in the state of fever and inanition, and less during convalescence than is excreted with a meat diet. Accordingly, these two final products, arising from the disintegration of the albuminous sub-

Virchow u. Hirsch, Jahresbericht fuer das Jahr, 18 Vol. I., p. 220.

stances, are excreted through the urine in a relatively larger quantity than in the normal condition. During convalescence, however, the urine also contains—in a larger quantity than during inanition—nitrogenous substances, not derived from disintegrated flesh, but from the nervous substance, that is, from a tissue, in which the nitrogen is not associated with sulphur. Besides, the albuminous substances supplied for the reconstruction of the wasted constituents of the body, are mostly used in the body itself, while the secretion of bile is simultaneously augmented. 4. The relative quantity of the phosphoric acid is less during the fever than with a meat diet; during convalescence and inanition, however, it is larger, though smaller than on feeding with brain. During the fever, therefore, and simultaneous with the augmented disintegration of the nitrogenous tissues, phosphoric acid is retained in the organism to be excreted after the disappearance of the fever. In inanition—together with the diminished disintegration of the nitrogenous constituents of the body—the relative quantity of phosphoric acid in the urine is augmented. 5. Accordingly, the exchange of muscular substance, is augmented during the fever, whilst in inanition, as in convalescence, the exchange affects more the nervous tissue, which, in virtue of its richness in lecithin and cephalin (Thudichum) furnishes comparatively less nitrogen, but more phosphoric acid. 6. These processes in the nervous substance cannot be referred to a simple increase or decrease, as, during the state of inanition, according to the law of *Voit* concerning the loss of tissue during the state of hunger, the total mass of nervous substance experiences the least loss. The regular change from retention to augmented secretion of the phosphates under existing and depressing influences, as also the widely varying results of the elementary analysis of the brain, rather indicate that the quality of the constituents of the tissues may experience rapid and intense changes."

The elevated temperature of the body in fever has usually been attributed to the increased disintegration or waste of tissue taking place during that process and liberating a surplus quantity of heat. The more recent investigations, cited above, however, have shown that the heat developed from this surplus exchange

of matter is not sufficient to account for that discharged from the surface of the patient, and that there must be other sources besides. The investigations of Leyden and Senator, particularly, have furthermore demonstrated that the discharge of heat during the whole course of the fever, though augmented, is nevertheless subject to considerable variations. The quantity of heat stands in no fixed proportion with the temperature of those accessible parts of the body, upon which it is usually measured, but may be less with a high temperature than with a lower, and even sink to the normal standard; in the stage of defervesence, especially when a critical sweat occurs, the discharge of heat is highest, amounting to double and triple the normal quantity. During the hot stage, it is augmented to one and a half, or nearly double the normal amount.

In order to understand these variations existing between the formation and discharge of heat, we need only examine the particular mode in which the healthy organism regulates these processes in keeping up the equilibrium of heat. As the heat, derived from the exchange of matter constantly occurring in every tissue of the body, is distributed throughout the latter by the circulating blood, it follows that the richer any part or organ is in blood, the higher will be its temperature; but as the amount of blood which an organ contains is proportionate to the number and caliber of its minute blood-vessels, the arterioles, venules and capillaries, its temperature will change with the contraction or dilatation of these vessels. In the normal condition, the tonus of these vessels, especially that of the arterioles possessing muscular fibers, is kept up by nerves—vaso-motor—connected with certain nerve-centers, chiefly placed in the middle portion of the medulla oblongata, but also in the spinal marrow; it is these centers, therefore, that regulate the normal temperature of the body by the amount of contraction or relaxation which they impose upon the muscular fibers in the walls of the vessels, or, in other words, by the amount of blood which they suffer to pass through the organ. The whole apparatus is reflex in its nature, and set and kept in operation by the impressions made, particularly upon the external surface of the skin; and it is thus that the application of cold will contract the minute vessels of the latter,

while moderate heat will cause them to become relaxed. To a certain extent, however, the discharge of heat from the body is also regulated by the lungs, the respirations increasing or diminishing in frequency; this is especially the case in animals clothed with a fur. Even in the normal condition, the quantity of heat produced within the body is not always the same, as, for instance, during the process of digestion, when by the more extended decombination and combination of matter taking place, a surplus of heat is produced; or, also, during muscular exercise, when more muscular substance is disintegrated than in a condition of quietude and repose. With man, the limits of this capacity of regulating the temperature in the interior of the body are quite narrow, lying between 27°–37° C. of the surrounding air; whatever is required beyond he has to replace by accessory means, as clothes, etc.

Now, as regards the discharge of heat during the febrile process, it is not augmented in the beginning or cold stage, but rather diminished; in the hot stage, or at the height of the fever, however, it is augmented 70–75 per cent. in the average during the day, and still more at the critical defervescence. As in the normal condition, the most heat is lost during the hot stage by conduction and radiation, while at the critical defervescence the discharge takes place by evaporation.

It has already been remarked that the augmented exchange of matter, indicated by the final products, urea and carbonic acid, does not account for the surplus of heat present in the body during the fever, and that there must be some other sources of heat besides. The most prominent of the latter are, according to Senator: 1. The consumption of those static forces, which, in the healthy organism, are stored up for the performance of any work. 2. The accumulation of heat during the pyrogenetic or cold stage of the fever. Besides these, other sources for the increased formation of heat may be sought in the greater disintegration and metamorphosis of albumen into urea, and in unknown processes concerning the formation of water.

With regard to the first of these sources, Senator presumes that the healthy body is in possession of latent forces, which are not generally liberated, but kept in store for extraordinary oppor-

tunities, such as an interrupted supply of nutriment, when they may be used, both for the performance of mechanical work, or for the development of heat. But as during the fever no external work of any import is done, these static forces are used up in the form of heat. Instead that in the healthy condition they are by the excitation of the will converted into mechanical work and heat, they will, on the other hand, under the influence of the febrile cause, be converted into heat only.

In the consumption of these static forces, which may either take place rapidly by mechanical work, or more slowly by hunger, carbonic acid is developed by the combustion of certain non-nitrogenous substances, containing these forces. The same should be the case, if the consumption of the latter takes place during the fever, to form a source for an augmented formation of carbonic acid during this process. When, nevertheless, such a source cannot be proved, or is not very conspicuous, even if present, no objection can be raised against the presumption that these static forces are liberated. For the surplus of carbonic acid formed here may be counterbalanced on the other side by being formed in a smaller quantity in such a manner that the albumen is not, as in the normal condition, completely consumed, but that it leaves behind, after the splitting off of the urea, a non-nitrogenous rest, more or less related to fat—the fatty degeneration of the parenchymæ in severe febrile diseases corroborates this hypothesis. The difference between the exchange of matter in a healthy and in a fevered individual is as follows: The healthy individual converts, under ordinary conditions, daily, a certain quantity of albumen and fat into the corresponding quantity of urea and carbonic acid, and replaces the loss by an equal quantity of fresh supply, keeping thus his store of static forces at the same level. If the fresh supply is insufficient, that is, during a more or less perfect state of hunger, then he consumes, in proportion to his state of nutrition, a quantity of albumen and fat of his body, and his store of static forces decreases. But the combustion is always perfect, that is, the quantity of albumen and fat destined to the exchange is completely converted into urea, carbonic acid and water. During work, he consumes the same quantity of albumen, with the only difference, that the static

forces, which during the state of hunger are more gradually consumed, are here liberated in a short time, giving rise to a rapid development of carbonic acid, which otherwise would have been discharged very gradually. During the fever, a larger quantity of albumen is disintegrated as during inanition, but only to urea and fat. The static forces are, perhaps in consequence of the enormous destruction of albumen, more rapidly consumed, and, for this reason, more heat and carbonic acid result in an equal time from this consumption than in a state of hunger; the carbonic acid, however, which would have resulted from the complete combustion of the albumen, is left out, so that, on the whole, nothing, or only very little more than in the corresponding time of hunger, is formed.

As regards the accumulation of heat during the cold stage, it is presumed that it really occurs. Even the normal discharge of heat during this stage would result in a saving or accumulation of it, because the changes in the exchange of matter are already taking place previously to the commencement of the chill, and as conditions for an augmented formation of heat may be found, besides, in the phenomena accompanying the chill itself. This presumption is moreover corroborated by the commencing augmentation of the discharge of urea, and the already preceding sensation of weakness and depression—phenomena which most probably depend upon the augmentation of the disintegration of albumen just beginning, and the consumption of the static forces in store. The spasmodic contraction of the blood vessels of the skin, together with that of the smooth muscular fibers, causing the "goose skin," and even of voluntary muscles—causing the chattering of the teeth,—must give rise to a development of heat. In the beginning of febrile diseases, there is always a period to be found in which the morbid changes, the elevation of the interior temperature, the augmented secretion of urea and other changes in the urine, the increased frequency of the pulse and of respiration, and, finally, various subjective sufferings, show the existence of the febrile process, and with it the augmented formation of heat, even in the absence of the normal heat of the skin, or without an augmentation in its discharge; a period, therefore, in which an accumulation of heat may be presumed to take place.

The variations in the quantity of heat discharged, occurring during the fever, do not depend upon a total loss of the capacity of the skin to regulate the temperature of the body, but are rather owing to an abnormal excitability and irritation of the cutaneous vessels, brought about by the influence of the original cause of the fever, and probably reflected to the respective nerve-centers. It is the irregular and unequal contraction of these vessels, therefore, which prevent the equalization of the surplus of heat present. This irregularity of contraction and dilatation of these vessels during the febrile process has been satisfactorily proved by Senator and other investigators, who carefully observed it on the vessels of the ears of fevered rabbits and dogs; and it explains the alternate sensations of heat and cold, experienced by fever patients, especially before the fever is completely established.

From the above statements, concerning the elevation of temperature during the febrile process, it may be concluded that it is brought about by a disproportion between the abnormally augmented formation and discharge of heat; though the discharge at the height of the fever may be greater than normal, and at times even greater than the febrile formation of heat. The disproportion, therefore, is not equally prominent in each phase of the fever, and it is supposed that every hot stage has been preceded by a pyrogenetic, or cold stage, for the accumulation of the heat, just as, on the other hand, it terminates by a stage of defervescence with an augmented discharge of heat. In mild cases of fever, the same variations occur in the discharge of heat, only more gradually and less intense.

Of late years, numerous additional investigations, regarding the febrile process have been made, chiefly for the purpose of determining the exact relation existing between the central and peripheral temperature, and, also, of the relative temperature of different parts of the external surface of the body itself, as, for instance, that of the right or left side of the thorax during febrile diseases of the respiratory organs. In these investigations, the temperature of the axilla, or, in some instances, of the rectum, was taken as a standard of the internal heat, while the skin between the toes, or other parts of the surface were chosen

for measuring the external temperature. But, though the results of these investigations, in general, corroborated the statements of Senator, and with it the neuro-pathological theory of Virchow, some contradictory statements have not been wanting. Thus, *Murri*,* basing his views upon the results of numerous experiments on animals, denied any dependence of the febrile process from the nervous system as chief regulator of the animal heat, and attempted to establish a bio-chemical hypothesis of fever in place of the neuro-pathological, now prevailing. He especially asserted that, in most cases of fever, no abnormal proportion between the peripheral and central temperature could be shown, and that the difference beween these temperatures were neither more variable, nor much greater than in the normal condition; but, on the contrary, often, and for a long time, remained unchanged during the course of the disease; frequently, even, the surface would be warmer than the central organs. *Jacobson*,† who experimented on a number of fever cases of pericarditis, pleuritis, typhoid fever, pneumonia, articular rheumatism, and tertiary intermittent fever, made use of a thermo-electrical apparatus. For the determination of the peripheral temperature, he introduced electrodes, in the form of fine needles, beneath the upper layer of the epidermis upon different parts of the cutaneous surface; for the central temperature the axilla was chosen. The results of these examinations were very inconstant. While, at one time, the difference of temperature between the axilla and the upper layers of the cutis was less during the fever than during the apyrexia, it would be greater at other times. Thus it varied irregularly between different places of the skin in the same individual, for, while the thermometer showed one and the same degree of temperature in the axilla, that of the skin was observed to undergo very considerable variations. The difference between the central and peripheral temperature was found to be in no way less than during the apyrexia. The same results were obtained in measuring the comparative temperature of the mouth and skin. These observations showed that at the acme of the hot stage, also, an alternate contraction and dilatation of the

* L. c. für das Jahr 1875, Vol. I, p. 281.

† L. c.

cutaneous blood-vessels takes place, and that during this stage, the quantity of blood in these vessels at the periphery of the body, and, in consequence, also the discharge of heat, are subject to variations within extended limits not only at different times, but simultaneously on different places. *Schuelein*,* with the view of answering the same question, instituted a series of thermometrical examinations on fever patients, for which he had availed himself of maximal thermometers with very small cylinders, corresponding very accurately with each other, and of which the one was introduced in the closed axilla, while the other was put between the first and second toe in such a manner as to completely embrace the bulb containing the mercury. The patients were quietly lying in bed under a light cover; the temperature of the room was almost always the same, varying with some exceptions, between 18.0° and 20.0° C. In healthy individuals, it was found that while the temperature of the axilla remained nearly constant, that between the toes showed great variations. In the course of typhoid fever, peritonitis, acute articular rheumatism, erysipelas, endometritis, miliary tuberculosis, and cheesy pneumonia, continuous variations of temperature of the skin, not corresponding with those of the axilla, took place. In typhoid fever, they were observed within considerably wide limits, even when measured every quarter of an hour. During a severe frost a descent of the temperature of the skin nearly coincided with a rise of that in the axilla. Another series of observations, the results of which corroborated the statements of Jacobson and Schuelein, were made by *Schuck*.† *Wegscheider*,‡ who made his examinations on pneumonia and typhous patients, states, that the temperature of the periphery was subject to great variations, not running in any way parallel with those of the axilla. They were greater during the fever than in the feverless condition, a fact which he regards as depending upon an abnormal state of excitation of the cutaneous vessels.

A number of other observations have been made on this subject, which it is needless to cite, as they likewise corroborate the

* L. c.
† L. c. für das Jahr 1877, Vol. I, p. 219.
‡ L. c.

variations occurring in the contraction and dilatation of the cutaneous vessels during the febrile process.

In reviewing the preceding sketch of the febrile process, it will be found that though the chemical and physical processes concerned were satisfactorily explained, nothing was said which could throw any light upon the primary cause inducing the abnormal phenomena characteristic of fever. But as, in order to form a correct idea of the general pathology of yellow fever, it is important to have a clear understanding, not only of the chemical and physical processes concerned in the febrile process in general, but also of the principal views which at the present time are entertained as to its primary cause, a brief discussion on the subject is demanded.

Judging from what is known about the chemistry and physics of the febrile process, as demonstrated in the preceding sketch, the most prominent phenomenon of the whole is the over-production of heat, depending chiefly, but not entirely, upon an increased disintegration and exchange of matter. This heat, however, though its formation may from the beginning of the fever be going on at an equal rate, is not equally distributed throughout and discharged from the body. In consequence, the elevated temperature in fever, as once said before, is brought about by a disproportion between the abnormally augmented formation and the not in the same degree augmented discharge of heat, though the discharge at the height of the fever may be greater than normal, and at times even greater than the febrile formation of it. But, while the chief cause of the latter is the augmented disintegration and exchange of matter, the cause of the unequal discharge of heat, depending upon an abnormal and irregular contraction of the cutaneous blood-vessels, must be looked for in the nervous system. The question to be answered in relation to the primary cause of fever, therefore, is whether the increased waste of matter in the organism is the result of the accumulation of the normal quantity of heat within the body, depending upon the diminished discharge from the surface, on account of the irregular and abnormal contraction of the blood-vessels of the skin,—or whether the augmented disintegration of matter, especially the albumen, preceded the contraction of these

vessels, and by some irritative influence upon the respective nerve-centers had perhaps, itself, been the cause of the contraction. In the one case, the process would have commenced in the nervous system, and be in concert with the neuro-pathological theory; in the other, its starting point would have been the blood, and it would correspond to the principles of humoral pathology. In examining the subject a little closer, it will be found that neither the one nor the other theory can be strictly and exclusively applied to every form of fever. For, in taking, for example, the febrile process as it accompanies the so-called infectious diseases which are supposed to depend upon a certain poisonous material entering the blood through the avenues of the lungs and the alimentary canal, it will be obvious that the process commences in the domains of humoral pathology; while, on the other hand, the febrile process following the shock of a severe bodily injury, and representing the so-called symptomatic fever, most probably took its start from the nervous system. Therefore, the primary cause of some fevers will be strictly humoral, whilst that of others will be nervous. But it must be remembered, that though in a number of fevers the effect of the cause, representing some noxious matter, is directly made upon the blood, it may also, through the medium of this liquid, very rapidly extend to the nervous system, which, reacting upon the one or other organ of the body, may induce serious pathological changes. And, in the reverse, in those fevers associated with severe surgical injuries, the disturbance and derangement in the nutrition of the injured parts may give rise to a certain pathological condition of the blood, which, through the medium of the nervous system, may conjure up the febrile process. We see, therefore, that the febrile process in general cannot be made exclusively to fit one or the other theory, but being of a complex nature, remains independent.

The neuro-pathological theory of *Virchow*, applied to the febrile process, explains many of the phenomena manifested, as, for instance, that of the irregular discharge of heat from the cutaneous surface by an irregular spasmodic contraction and dilatation of the cutaneous blood vessels, caused by a morbid irritation of the vaso-motor nerves. But, although the phenomenon of the

non-equalization of heat is of a nervous origin, it does not necessarily follow that the augmented disintegration and exchange of matter is likewise the direct result of a perverted nervous action, for it has, as yet, not been positively shown that the original production of animal heat depends solely upon the nervous system, unless we presume the existence of special nerve-centers performing the function of regulating the exchange of matter, and with it the production of heat. The experiments of *Magendie*, *Bernard*, *Buettner*, *Rollet*, *Mantigazza*, *Schiff*, *Vulpian*, *Obolensky*, *Haidenhain* and *Legros*, made on animals in relation to this subject, and the clinical observations of *Samuel*, *Weir*, *Mitchell*, *Morehouse*, *Keen*, *Charcot*, *Erb*, and others, on man, however, seem to indicate the existence of so-called "trophic" nerve-centers, which regulate the nutrition of the tissues and organs, though on the other hand, a number of other investigators have obtained negative results from their experiments. The decrease of heat in paralyzed limbs, also, points to the influence of the nervous system upon the exchange of matter in the paralyzed parts. A number of experiments, consisting in the section of the spinal marrow, have likewise been made on animals by *Bezots*, *Ludwig*, *Thiry*, *Tscheschichin*, *Schiff*, and others, for the purpose of determining the variations of temperature in the extremities, with other observations on cases of severe injuries of the spinal marrow of man; these, however, may be passed over, as the results obtained are to a certain extent contradictory, and only seem to indicate local disturbances in the function of the blood vessels.

Now, even in recognizing the doctrine of trophic nerve-centers and nerves, and in admitting that the exchange of matter in the organism, constantly taking place in the fluids and solids, depend upon nervous stimuli, a special cause will still be required to act as such, and, accordingly, it may be presumed that in a similar manner as the nerve-centers presiding over the circulatory and respiratory functions, are stimulated to their rythmical reflex actions by the relative quantity of oxygen and carbonic acid in the blood, the normal combinations and decombinations of matter, constantly taking place in the blood and tissues, may form a stimulus for the trophic centers. As long as the organism remains

undisturbed by external injuries, or by the influence of noxious substances, introduced into the blood from without, the trophic centers, then, will discharge their function in the same regular reflex manner as the respiratory; the exchanges of matter will take place in their just proportion, *i. e.*, each atom of matter will be replaced by an atom of fresh matter, and the normal nutrition of the tissue will be preserved. At the same time, the quantity of heat liberated by the exchange of matter will be counterbalanced by an equal discharge, regulated by the normal contraction and dilatation of the cutaneous vessels. But, as soon as any foreign substance, that is, a body incapable of being assimilated by the organism for the rejuvenation of the tissues, and therefore incapable of becoming a constituent of the latter, enters the blood, it will be removed by one or the other of the secretory organs, the only outlets from the organism. There are a large number of substances, some of which are used as medicines, which thus may enter and leave the organism without injury, and be detected in the secretion, while there are others, which, after entering, will, either catalytically, or by entering into combination with the organic constituents of the blood and tissues, exert a noxious or fatal influence upon it. To these belong the so-called infectious poisons with which we have to deal in this treatise.

In the symptomatic fevers, the morbid impression may be carried to the vaso-motor or trophic nerve-centers by the sensory nerves of the skin, or of the traumatic lesion, affecting them in the same manner as the respiratory centers are affected by sudden impressions made upon the skin, etc.; in the fever of infectious diseases, however, experience teaches that they are affected through the medium of the blood. Even Virchow, when founding the neuro-pathological theory, admitted this probability in saying:* "The rapidity with which febrile phenomena appear and disappear, likewise points to a cause residing in the nervous system, though the noxious material, affecting the nervous system, may be sought in the blood." And, further on: "In the study of the chemical nutritive processes of the body, it is of advantage to exclude the nerves as long as possible, as it has been shown that

* *Virchow.*—Handbuch der Speziellen Pathologie und Therapie, 1854, Vol. I, p. 36.

they exert no other influence, but in determining quantity or, acting as excitors or moderators, for all qualitative changes come from other directions."

In studying the general pathology of yellow fever, we must keep in mind that the clinical phenomena exhibited during its course, and the pathological changes met with after death, *do not*, as I have hinted at before, *exclusively* belong to this disease, but are equally observed, though in a more or less modified form, in other infectious, and even non-infectious diseases. It is only when they are considered as a totality, that they may represent the characteristic features of the disease. Thus, the continuous type of the febrile process witnessed in yellow fever, together with the congestion and parenchymatous infiltration and degeneration in various organs, are equally observed in other infectious diseases, particularly those of a contagious nature. The chief difference seems to lie in the specificity of the noxious poison, and in its tendency to affect particular organs in the different diseases, while its general effects upon the whole organism in these affections bear much resemblance to each other.

In the beginning of this treatise, I have already remarked that, in all cases of yellow fever, as well as of other infectious diseases, there is a prodromal stage, during which the poison exerts its noxious influence upon the blood, and primarily deranges the normal exchanges of matter. The symptoms of this stage, consisting, as we have seen, in a general discomfort, anorexia, lassitude, headache, etc., are decidedly nervous in character, from which fact it may be presumed that it is the nervous apparatus, which, first of all others, experiences the effects of the noxious poison. Now, although it is the blood with which the poison comes first into contact, it remains nevertheless difficult to determine whether it is directly carried by this fluid to the tissues of the nervous apparatus to make a direct impression upon them, or whether, in one way or other, it first contaminates the plasma of the blood, and, accordingly, makes an indirect impression upon the protoplasm of these tissues by deranging their normal nutrition. The latter supposition appears to be the most probable. At any rate, the influence which the poison at this period of the disease exerts upon the nervous system, is depressive in its nature, and

appears to be directed to the vaso-motor centers. The immediate effect of a depression of these centers is a relaxation of the muscular elements in the walls of the blood vessels, especially of the smaller arteries, followed by a dilatation of these vessels and a hyperæmic condition of a moderate degree, upon which the prodromal phenomena seem to depend. Judging from the degree of the headache, it appears that the hyperæmia of the brain, upon which it depends, is at this period principally confined to the pia-mater, though the minute vessels of the cortex cerebri, also, may be slightly over-filled with blood. The sympathetic ganglia likewise suffer from the effects of the poison, and, moreover, exert their depressing influence upon the functions of the liver, stomach and kidneys. The degree of the prodromal symptoms in the different cases, is obviously proportionate to the quantity and intensity of the poison absorbed, but also to the particular susceptibility to the noxious influence of the poison by the person infected. In persons very susceptible to the absorption and influence of the poison, and who are exposed to it in its intense form by proximity to a severe case of yellow fever, the prodromal stage, therefore, may be comparatively short, while in others, not as susceptible, and exposed to the poison in a milder form, the prodromal phenomena may be milder, and extended over a longer period of time. From this difference, observed in the severity and length of the prodromal stage in different cases, we may presume that the abnormal disintegration and exchange of matter in the organism, resulting from the effects of the poison, must have proceeded to a certain extent, before the febrile process can start into action. The augmented quantity of urea, found in the urine before an elevation of the normal temperature of the body can be perceived, shows the correctness of this supposition. But, while the first impression of the poison upon the nervous system is of a depressive character, a sudden change takes place with the true commencement of the febrile process, evidently owing to a certain reactive effort of the vaso-motor nerve-centers, and manifesting itself in the abnormal excitability and irritation of the cutaneous blood vessels. It is impossible to decide whether this irritation proceeds from the vaso-motor nerves of these vessels, or from the sensory nerves of the skin, to be propagated

to their respective nerve-centers, thus constituting a simple reflex action; or, whether it originates in the latter themselves, and is originally caused by the now concentrated effects of the infectious poison, or by the products arising from the augmented disintegration of albuminous substances, accumulated within the blood, and increasing the previously commenced disturbance of nutrition. The first effect of the augmented excitability of the vaso-motor nervous apparatus, then, is observed in the contraction of the cutaneous blood vessels, giving rise to the phenomena of the pyro-genetic stage, the mildness, severity, or duration of which evidently depends upon the quantity and intensity of the fever poison in the blood. Although those initiatory symptoms of the febrile process are subject to great variations in different cases of the disease, I do not think that they are ever entirely absent, but that, even in those cases in which no regular chill is observed, an alternate sensation of cold and heat, indicating the irritation of the respective nerve-centers, is experienced by the patient,—while in others, in which a regular chill, or even rigor, occurs, the spasmodic contraction of these blood vessels is more decidedly pronounced. As soon as the nervous energy, causing the contraction, is exhausted, a relaxation of the muscular fibers in the walls of the blood-vessels, followed by a dilatation, takes place, and in consequence of a larger quantity of blood coming from the central and warmer parts of the body, passing now through the vessels, the temperature of the skin gradually rises, until the hot stage of the fever is fully established.

In considering the disturbance of the nervous functions in yellow fever, we must not imagine that a depression or excitation of the nervous tissues takes place simultaneously throughout all parts of the nervous system, and that in consequence the blood vessels of all organs must be in a corresponding state of dilatation or contraction. On the contrary, as will be seen, the vessels of one part of the body may be relaxed, while those of another may have preserved their normal tonus, or, as in the case of the cutaneous vessels, may alternately be contracted or relaxed. In order to appreciate correctly the relative disturbances in the functions of the various organs of the body, and the pathological changes taking place in the blood and tissues during the whole course of

an infectious disease like yellow fever, one should be familiar with all the anatomical and physiological minute details of the entire organism, the study of which, alas! is rather neglected by the majority of practicing physicians. Without the knowledge of these details, which, after all, constitute the basis and greater portion of true medical science, the physician can only form an indefinite, vague idea of the normal and pathological processes taking place in the human organism, while, with it, he may represent to his mind the entire mechanism of the body in all its details, and in full operation.

Already, before the commencement of the hot stage, pains in the joints and limbs, as we have seen, are added to those in the head, increasing in severity with the rise of the temperature, until the fever is fully established, when they either disappear, or continue in a milder form during the hot stage. These pains, which are most severe in the lumbar region and pelvis, must probably depend, as mentioned once before, upon the congested condition of the blood vessels of the pia mater, and of the spinal marrow in the lumbar region, whence they are reflected to the joints and muscles affected. Whether these pains, as well as those in the head, are simply owing to the pressure of the congested blood vessels upon the nervous substance, or whether their severity partially depends upon the direct noxious influence of the infectious poison upon the nervous tissue of the affected parts, I will not venture to decide, though the latter supposition appears probable from the fact, that, in fatal cases, these pains generally disappear before death, while the blood vessels are still found congested with blood corpuscles after death.

The nervous apparatus, through the influence of which the circulation of the blood is sustained and regulated, is quite complex, and consists of several nerve-centers. While the rythmical movements of the heart, by which the blood is kept in circulation, are most probably caused by the influence of certain nervous ganglia situated in the muscular substance of the heart itself, they are regulated by other centers situated in the medulla oblongata, and operating through the pneumogastric and sympathetic nerves; the nerves which the heart receives from the cardiac plexus are derived from these sources. The fibers of these nerves, however,

differ very considerably in their function, for while the pneumogastric fibers transmit only inhibitory stimuli, the other transmit stimuli which accelerate the heart's action. Accordingly, an irritation of the fibers of the pneumogastric nerves will be followed by a retardation, or even arrest of the movements of this organ, while an irritation of the centers in the medulla oblongata will produce an acceleration, as long as their communication with the heart through the spinal marrow, rami communicantes, first thoracic ganglion, etc., is not interrupted. An acceleration of the heart's action will also be produced by an increased pressure of the blood within the cavities of the organ, as may be caused, for example, by a contraction of the smaller arteries, depending upon an irritation of the vaso-motor centers, likewise situated in the medulla oblongata. From this it is easy to understand how an irritation of the medulla oblongata, produced by the noxious influence of the yellow-fever poison, will give rise to one of the most prominent phenomena of the febrile process, the frequency of the pulse. Besides this, the elevation of temperature also, depending upon the augmented disintegration and exchange of matter, initiating the febrile process, forms another cause for the accelerated action of the heart. The latter assertion, however, must appear contradictory to the circumstances, before mentioned, of the falling of the pulse on the second day, a time when the temperature is still rising. But may not this phenomenon be owing to an irritation of the inhibitory centers of the pneumogastric nerves, set up at this time?

The abnormally augmented disintegration and exchanges of matter in the organism, originally caused by the presence of the infectious poison in the blood, and giving rise to an increased formation of urea and other products, as well as to an augmented quantity of heat, will now continue, until the poison is destroyed, or eliminated from the system, while, at the same time the products arising from these abnormal processes, or the direct action of the poison itself upon the nervous tissues, or upon the morphological elements of the blood, may give rise to the abnormal excitability and irritation of the involved nerve-centers with their nerves.

The morbid excitability of the nervous tissues, however, does not appear to be confined to the centers and nerves already mentioned, but, moreover, extends to other parts of the nervous system, giving rise to other phenomena of the hot stage, consisting in the congestion and deranged secretion of several organs. Thus, the congestion of the blood vessels of the conjunctiva very likely depends upon a neuro-paralysis of the vaso-motor nerves, and is similar in character, though inferior in degree, to that produced in the blood vessels of the rabbit's ear by the well known experiments of Claude Bernard of dividing the sympathetic nerve in the cervical region. The deeper seated pains of the eye, and also the slight photophobia, are phenomena which depend upon the irritation of the trigeminal nerves and their centers. In the same manner, the congestion and irritation of the mucous membrane of the pharynx, œsophagus, and stomach are explained; for in considering the fact that all the minute blood vessels of the brain, but particularly those of the pons varolii, corpora quadrigemina, and medulla oblongata, are found after death in a congested condition, completely filled with blood corpuscles, there will be no difficulty in understanding the congestion and irritation of these mucous membranes, if we remember that the nuclei, or centers, of the trigeminal, glosso-pharyngeal and pneumogastric nerves, supplying these parts, are situated near each other, alongside of the aqueduct of Sylvius and the floor of the fourth ventricle of the brain, and that all these nerves closely communicate with the sympathetic. Judging from the scarlet-red color of the congested vessels of the conjunctiva, gums and edges of the tongue, during the first part of the hot stage, the congestion is arterial in its nature, that is, depending upon a relaxation of the arterioles and the ensuing afflux of blood. As regards the stomach, it hardly needs mentioning that the nausea and inclination to vomiting are due to the irritation of its mucous membrane, the secretory function of which, however, is not suspended, but only deranged, as shown by the quantity of frothy mucoid liquid thrown up at this time. As regards the liver, kidneys and supra-renal bodies, I have already mentioned, when discussing the pathological anatomy of these organs, that the parenchymatous degenerations, observed after death, have been initiated by a

congestion of the blood vessels, depending most probably upon the same causes as that of the mucous membranes above discussed. But, before passing to the discussion of these parenchymatous changes, I must make some remarks on the condition of the blood.

In the section of this treatise devoted to the pathological anatomy of the blood and a number of organs, I showed that, besides an unusual tendency of assuming the mulberry or thorn-apple forms, manifested by the colored blood corpuscles, nothing abnormal, or foreign, could be detected in this fluid, when carefully examined directly after it had been removed from the living patient; while, on the other hand, in almost every organ in which a congestion had occurred, numerous extravasations or infiltrations of the coloring matter of the blood corpuscles, the hæmo globin, into the parenchyma were met with. The question arises, therefore, whether the hæmoglobin parted from the colored blood corpuscles only in those places where the extravasations occurred, by virtue of the existing retardation of the circulation in the congested capillary vessels, or whether it escaped from the corpuscles while they were still in an active circulation. In answering this question, it may be said, that the direct cause of these extravasations evidently was the retarded circulation, but that there also existed a remote cause, consisting in a want of coherence between the hæmoglobin and the protoplasm of the blood corpuscles, facilitating, as soon as the retardation took place, the escape of the former from the latter. And, it may further be presumed, that not all the hæmoglobin escaping from the blood corpuscles, passed through the walls of the vessels, but that perhaps the greater portion became mixed with the liquor sanguinis and was carried into the general circulation, giving rise to the jaundice. Thus, it will be seen, *that the icterus in yellow fever is not owing to the presence of bile in the blood*, as is believed by a large number of physicians, *but to the presence of free hæmoglobin*, and represents in truth the so-called "*hematogenous*" jaundice. A "*hepatogenous*" jaundice cannot take place, as the larger as well as the smaller hepatic ducts are found perfectly open, and as the secretion of bile during the disease, in the majority of cases, is rather diminished, or even suspended.

As regards the presence of free hæmoglobin in the blood, however, some doubt might still be expressed on account of its not having been demonstrated by the microscopical examination of so many specimens of fresh blood, taken from the living patients. The reason for this failure is, that the quantity of hæmoglobin contained in the exceedingly thin layer of blood required by microscopical examination, is too small to exhibit its yellow color to the eye of the observer, and that in order to be seen, the blood should be saturated with the coloring material to such a degree as would require a quantity larger than would be consistent with life. Free hæmoglobin, therefore, may be present in the circulating blood without being detected by the microscope. The faintly yellowish tint, appearing, in a number of cases, as early as the third day of the disease, in the conjunctiva and the skin, corroborates this assertion; though, in the majority of cases, the jaundice appears later, if it appears at all during life. The yellowish tint of the conjunctiva and skin, however, is not produced by the free hæmoglobin still contained in the circulating blood, as its presence is not perceived until it has escaped from this fluid into the juices of the surrounding tissues, to be absorbed by the cells of the epithelium of the conjunctiva, or those forming the lowermost stratum of the epidermis of the skin.

The extravasations of hæmoglobin into the parenchyma of various organs, which I have observed to occur in yellow fever, fully corroborate the generally accepted fact mentioned before, that in the febrile process, it is these nitrogenous tissues, rich in potassa and hæmoglobin, especially the colored blood corpuscles and muscles, which are most prone to disintegration, and which first suffer from the effect of the poison.

Let us now direct our attention to the liver. Although in most cases of yellow fever, as I have shown, the traces of congestion of the blood vessels, observed after death in this organ, are not as conspicuous as in other organs, there remains for the reason already given, no doubt but that the liver is one of the first organs which experience the deleterious effects of the poison, and participates in the general hyperæmia resulting from the depression of the vaso-motor nerves, manifested during the prodromal stage. But, like in the stomach, as soon as reaction

takes place, the secretion of bile is resumed, and, very likely, for a short time abormally increased, as may be inferred from the vomiting of bilious matters, observed in a number of cases during the commencement of the disease. Judging from the fact that most cases of miasmatic fevers, even the simple intermittent, are accompanied by some derangement of the biliary functions, I have always been inclined to regard the liver as that organ through which the organism makes the first attempt at eliminating the miasmatic poison by stimulating its secretory function. The same may take place in yellow fever, as above mentioned, though the abnormally stimulated secretory function will soon be exhausted, and an opposite condition prevail. But, besides this stimulation through the secretory nerves, it appears very probable that the liver receives another stimulus, proceeding from the part of the blood, for the purpose of removing, and converting into bilirubin, the free hæmoglobin escaped from the colored blood corpuscles. But like even in the normal condition, the amount of labor which the parenchyma of a gland can perform, stands in a certain proportion to the quantity or number of its secreting cells, only a small portion of the free hæmoglobin can be converted into bilirubin by the liver, while the rest extravasates through the capillaries of the different organs and tissues as we have seen, to be absorbed by the surrounding cells, or to mingle with the fluids of the tissues. Thus, the abnormal stimulation of this organ, accompanied by the increasing disturbances of the nutrition of the whole organism, soon leads to a state of exhaustion.

But, while these functional changes are taking place in the liver, the abnormal processes in the blood, relating to the augmented disintegration of the albuminous constitutents, and their metamorphosis into the final products urea, carbonic acid and water, are likewise running their course. The albumen, thus undergoing disintegration, may be derived from two sources, *i. e.*, from the circulating albumen of the blood, designated by *Voit* the "store albumen," or from the albumen of the tissues, the "stable" or "tissue-albumen." As the former, on account of the small quantity, is soon consumed, it is especially the latter which suffers the greatest losses during the febrile process. In consequence of the augmented formation of urea, consuming consider-

ably more oxygen than is normal, the other final products, requiring still more oxygen for their formation, must fall short in quantity, and the combustion of the non-nitrogenous constituents be retarded. But, as an abnormal quantity of albumen is decomposed without a corresponding quantity of non-nitrogenous material, such as fat, the latter accumulates in the blood, and gives rise to the *fatty infiltration* of the different organs, already described.

Fatty infiltration of the liver, besides being met with in infectious diseases, also frequently occurs in chronic diseases accompanied by great exhaustion of the system, such as phthisis, chronic diarrhœa, dysentery, etc., in which the normal processes of nutrition and assimilation are severely deranged, so that a part of the albumen, destined for the rejuvenation of the tissues, instead of being appropriated for this purpose, becomes converted into fat; the same occurs through the imperfect combustion of the non-nitrogenous substances in the lungs. Generally, in these diseases, the nutrition becomes deranged only gradually, and, if fatty infiltration of the liver takes place, the process advances, in most instances, at the same slow rate of the original disease. In yellow fever, however, the case presents a different feature. Through the deleterious influence of the infectious poison, the disintegration of albumen and the exchanges of matter take place quite rapidly, and the fatty infiltration of the liver may commence at a comparatively early period of the disease, and, in advancing, seriously interferes with the neighboring organs. For, besides the diminution and final suspension of the biliary secretion which it causes, it also interferes with the portal circulation by the pressure, which the abnormal amount of fat, deposited by the blood and absorbed by the hepatic cells, exerts upon the capillaries and inter-lobular vessels. And it is this pressure by which the congestion, representing the first stage of the pathological process in this organ, is gradually reduced. In advanced cases of fatty infiltration, therefore, the minute vessels are very rarely found over-filled with blood; on the contrary, the capillaries, as we have seen, are mostly found empty. The disturbance in the portal circulation, however, will soon be felt by the tributaries of the portal vein, for, while the passage of the

blood through the liver is rendered more difficult, a congestion of these veins will be the result, which, in extending backward to the minute venules of the mucous membrane of the stomach and intestines, will give rise to that peculiar congestion of these organs, already described. In the stomach, this congestion is mostly accompanied by an infiltration of hæmoglobin into the surrounding tissues; and, in severe cases, ruptures of the minute venules, or capillaries, situated between the epithelial and glandular layers, will occur, and give rise to that much dreaded clinical phenomenon, known as "black vomit."

The vomiting of black matters in yellow fever has always been regarded as a most unfavorable symptom. The morphological elements of the blood which these matters contain are derived, as we have seen, from the minute hæmorrhages caused by the rupture of the vessels just mentioned. It is quite obvious that such a hæmorrhage must result in the direct relief of these minute vessels from their congested condition, and that the small quantity of blood, thus lost, is in itself inadequate to cause the fatal issue of the case, though *it most certainly indicates a very diseased condition of the liver, as well as a low state of the whole organism;* and it is, in this respect, that it becomes an important factor in the prognosis of the case. Besides this, although the primary congestion and irritation of the stomach is, during the first three days, rarely accompanied by much pain, when pressure is made upon the epigastrium,—it will finally, when aggravated by the advancing fatty infiltration of the liver in the manner described, give rise to a sensation of dull pain and fullness in the organ, rendering pressure insupportable to the patient. The condition of the liver, and with it that of the whole organism, therefore, may be accurately diagnosed by that of the stomach, and will notify the physician of the approaching danger. Very rarely black vomit occurs before the fourth, or even fifth day; in most cases, perhaps, it takes place still later, at a time when the pathological changes in the various organs have attained a high degree, and when the nervous energy of the organism is nearly wasted, producing an almost complete depression and exhaustion of the patient.

If, after the elimination of the poison from the system, indicated by the cessation of the febrile process, the pathological changes of the parenchyma of the liver have not advanced to a very great extent; that is, if the protoplasm of the hepatic cells itself, has not undergone *true fatty degeneration*, and, if sufficient energy is left in the nervous organs to supply the wants of the organism—*black vomit may occur without proving fatal.* For, after the removal of the disturbing cause, the poison, the condition of the blood will gradually attain again its normal standard, the tissues will again be properly nourished, and, in consequence, the deposition of fatty matters into the parenchyma of the liver will cease, and, finally, the fat in the hepatic cells be reabsorbed by the blood. In this manner, quite a number of patients recover from black vomit. In fact, in most cases in which black vomit occurs, the patient feels relieved after the ejection of these matters, and imagines that he will get well. In children, especially, the occurrence of black vomit is not as often followed by a fatal issue as in adults. This is owing to the exchanges of matter, especially in those organs pertaining to organic life, being more active than in adult life; while, on the other hand, the organs of animal life, particularly those of the nervous system, are in children more impressible and sensitive.

In many fatal cases of yellow fever, especially those in which the third stage is protracted, the pathological changes of the liver do not remain confined to a simple *fatty infiltration*, but advance to a *true fatty degeneration* of the protoplasm of the hepatic cells, as I have described in the section of pathological anatomy. These cases, of course, as may be expected, prove necessarily fatal.

The relationship existing between the stomach and the liver, also exists between the latter and the intestines, and it is therefore natural that the same pathological phenomena should be observed in these organs, though inferior in degree. Accordingly, in a number of cases, hæmorrhages, similar to black vomit, are observed to take place from the mucous membrane of the small intestines, which, mingling with the mucous matters, pass per anum, though, sometimes, the black matters from the stomach may also be voided in this direction.

The infiltration and degeneration of the liver, associated with congestion and minute hæmorrhages of the stomach—black vomit—are so constantly met with in every fatal case of yellow fever, that they may safely be regarded as a characteristic phenomenon of this disease. In cases in which the black matters are not ejected during life, they will, almost always, be found in the system after death.

In taking now a glance at the kidneys, the pathological changes of which have already been fully discussed in their appropriate place, we find that these changes have, as in those organs already discussed, been preceded by a hyperæmic condition primarily depending upon the same cause, that is, a partial paralysis, or paresis, of the vaso-motor nerves. One of the first phenomena, depending directly upon the relaxed condition of the minute vessels, especially the arterioles, is the albuminuria, making its appearance quite early, on the second or third day of the disease. It is now generally accepted that this phenomenon, very frequently accompanying even mere functional disorders of the kidneys, chiefly depends upon an increased pressure of the blood upon the inner surface of the walls of the minute vessels of the glomeruli, as has already been noted. In yellow fever, the conditions necessary to the production of albuminuria, are rendered especially favorable by the relaxed state of the smaller arteries, so that, soon after the commencement of the febrile process, when the heart's action is increased, albumen may make its appearance in the urine, without being accompanied by other products indicative of organic changes in the organ. The degenerative processes commence at a later period, during the second stage of the disease, when the increased disintegration of albumen and exchange of matter, entailing a complete derangement of the nutrition of the various organs, has reached a higher degree. As in the liver, a slight reaction, resulting in a temporary stimulation of the secretory functions and following the general nervous depression, also takes place most probably in the kidneys. This stimulation may be caused by the accumulation of urea, or its components, in the blood, calling upon the kidneys for elimination from the organism. And it seems to be not impossible that this same cause, together with the deranged nutrition of the

epithelial cells, lining the uriniferous tubules, may also give rise to that abnormal product of secretion of which the cylindrical infarctions are formed. The nature of this abnormal secretion. and the manner in which the different kinds of cylinders are formed, have already been so thoroughly discussed as to require no further remarks in this place.

It has already been mentioned that the degenerative changes in the parenchyma of the kidneys are not observed to have taken place in the same degree in all fatal cases of yellow fever, but, on the contrary, differ widely from each other. From this fact, it may be presumed, that in those cases in which these changes have been limited, death cannot be assigned to them, even if the urinary secretion should have been suspended for an abnormal length of time before this event took place. Even in those cases in which these changes have progressed to a considerable extent, there is a sufficient number of uriniferous tubules left open for the passage of the secreted urine. And if, in some cases, a total suppression of urine really takes place, it is hardly due to the obstruction caused by the infarctions, but much more probably by the general *atrophy* and *degeneration* of the epithelium. But, even if this were the case, the question whether the suppression of urine is the immediate cause of death, as is believed by a large number of physicians, remains still undecided.

The question whether or not the nervous symptoms accompanying certain organic changes in the parenchyma of the kidneys, and called "uræmic," really depend upon an accumulation of urea in the blood, has been agitated for a considerable number of years, and is as yet not definitely settled, though, in the course of this time several theories, in a modified form, have been advanced. Thus, while a number of physicians regard the accumulated urea in the blood as the immediate cause of uræmia, others accept the theory of *Frerichs*, according to which these nervous phenomena are owing to the carbonate of ammonia resulting from the decomposition of the urea in the blood; while others, again, attribute them to the presence of the extractive matters. *Traube*, even, rejected these theories altogether, and attributed the symptoms of uræmia, consisting in convulsions, coma, vertigo, etc., to œdema of the brain. Since these different theories

were advanced, a considerable number of experiments and observations have been made in relation to the subject, for the purpose of ascertaining the truth, and almost all the results obtained rather prove the fallacy of the uræmic theory. I shall cite the views of some of these investigators. Thus, *Chalvet** found during the uræmic attack a diminution of the urea in the urine and blood, rising again after the attack, but without attaining the normal quantity. Accordingly, he rejected the theory of Wilson and Frerichs, based upon a retention of urea in the blood, and rather regarded the retention of excremental matters in general as the cause of the uræmic phenomena. He, moreover, doubted that urea was formed in the tissues at all, but were rather a secretory product of the kidneys. *Budde*,† who made quite extensive observations on this subject, opposes the uræmic theory based upon the poisoning by urea, and particularly points to the fact, that a diminution of the secretion of urea before and during the uræmic attack does not constantly occur. He says that it is true that such a diminution takes place with a simultaneous decrease of diuresis, though the latter is in many cases rather abundant during the uræmic state. Four cases, which the author himself closely observed and described, showed an abundant diuresis, and three of them, besides, an excretion of urea in a comparatively large quantity, namely: 25.7, 27.6 and 22 grms. In 25 severe and fatal cases of uræmia, which he collected, there were five with abundant diuresis up to the commencement of the attack, in one of which the diuresis even rose to from 1200–2000 Ccm. during the attack. The conclusion drawn from this is, that, even if the excretion of urea is considerably diminished, or temporarily ceases altogether, uræmia must not necessarily follow; it rarely occurs in amyloid degeneration of the kidneys, neither in a cachexia, as leucæmia, notwithstanding the very scanty excretion of urine in these cases. Finally, uræmia does not necessarily follow upon long persisting anuria, accompanying certain diseases of the kidney and urinary passages. Budde observed himself a case of anuria lasting 40 hours, not followed

* *Virchow u Hirsch*—Jahresbericht f. d. Jahr, 1867, Vol. I, p. 336, and 1868, Vol. I. p. 226.

† L. c.—f. d. Jahr, 1874, Vol. I, p. 347.

by uræmia;* he furthermore reports a case of a woman, of 24 years, with highly albuminous and abundant urine—during several ca. 1700 Ccm., a few times even to 2300 Ccm.—and considerable œdema, in whom slight uræmic symptoms were observed, disappearing again with the decrease of the œdema. Two weeks previous to death, severe diarrhœa and vomiting, accompanied by scanty urine and disappearance of the œdema, occurred; and in the last 132 hours total anuria with no discharge per os or per anum, and without any uræmic symptoms. The autopsy revealed: chronic nephritis in the second stage, amyloid degeneration of the spleen, bronchiectasis and phthisis cavernosus pulmonalis. The author then shows the untenability of the theory, according to which uræmia is caused by a poisoning through the extractive matters, or through the carbonate of ammonia, and endeavors to prove the incorrectness of the assertion of *Jaccoud*, that toxic uræmia clinically differs from that depending upon anæmia and œdema of the brain, and that the former can be recognized by an œdema otherwise wanting, as he found in his 25 cases of fatal uræmia six with œdema and anæmia of the brain without œdema in other localities, and six others with the same condition of the brain, and inconsiderable œdema in other places. For this reason, Budde sanctions the theory of *Traube*, which assigns œdema and anæmia of the brain as the cause of uræmic phenomena. In order to show how frequent these changes in the brain are met with, he collected and tabled these 25 cases of fatal uræmia which he observed in the Communal Hospital of Copenhagen. In 18 of these cases, anæmia and œdema of the brain were found; in four others, accumulations of liquid in the ventricles and in the subarachnoid spaces of the brain were present; in two more there was anæmia alone, and in one only, the brain was normal. These statistics support the theory of Traube in a high degree.

More recent experiments, consisting in the injection of urea into the blood, have neither been followed by convulsions, but

* A remarkable case of suppression of urine, observed by my friends, Drs. D. W. Brickell and J. D. Bruns, occurred very recently in this city. It was a boy, nine years old, who suffered from congestive bilious remittent fever, with a complete suppression of urine—duly ascertained by the use of the catheter—for 67 hours previous to death. His intelligence was perfect up to the instant of death, and no trace of uræmia was observed.

rather by a rapid excretion of the urea, proving what has been said before.

From the observations, cited above, together with others that occurred in this city, we have reason to judge that in those rare cases of yellow fever, in which a total suppression of urine really takes place, we are not justified in assigning it as the true cause of the convulsions, occasionally observed before death. A suppression of urine, therefore, can only signify an extensive degeneration of the secretory structure of the kidneys, the epithelium, and an exhausted condition of the nervous system, similar to the black vomit, indicating the desperate condition of the liver. At the same time, it must be remembered, that the secretory function of the kidneys is really not totally suppressed in all cases in which suppression is reported, but that, if they were properly examined, many of them would turn out to be only cases of retention, while in others the urinary function would be found to be only temporarily suspended. Thus it has frequently occurred that in cases in which the physician had even based his judgment upon the results obtained from the use of the catheter when pronouncing suppression, the secretion of urine had only been suspended, and was resumed soon afterward.* In many cases, however, suppression of urine is pronounced without the catheter ever having been resorted to. The fact that urine is found after death in the bladder in, at least, half the cases of autopsies, corroborates this assertion. We may then reasonably suppose, that though total suppression of the urine may take place in exceptional cases, it is not the immediate cause of convulsions and death, but that, on the contrary, the latter are owing to the diseased condition of the brain.

In the preceding section of this treatise I have shown, that in the numerous fatal cases of yellow fever in which I examined the brain, not only the smaller blood-vessels of the pia mater but also those of the brain substance itself, were found congested with blood, and that, in almost all of these cases, the congestion extended throughout the whole organ, and particularly through-

* *Dr. J. Dickson Bruns* reported to me a case, which he observed during the epidemic of 1873, and in which the secretion of urine was thus suspended for 36 hours, at the expiration of which time it was fully resumed.

out the pons varolii and medulla oblongata, in which the vaso-motor, trophic and secretory nerve-centers are situated. The hyperæmia of the brain depends upon the same cause as that of other organs, that is, upon a depression of the vaso-motor nerves, followed by a relaxation of the arterial walls. Gradually commencing before the beginning of the fever, it manifests itself already in the headache and general nervous depression during the preliminary or initial stage of the disease, but increases with the appearance of the fever, when the action of the heart becomes more violent, and when the arteries, in consequence of the relaxed condition of their walls, are no more able to withstand the increased pressure of the blood. And as these vessels cannot recover their normal tonicity as long as the battle between the organism and the infectious poison is raging, the congestion of the brain becomes persistent, and increases the already existing disturbance of the nutrition of the nervous tissues of this organ. In consequence, a considerable amount of the nervous energy, which in the normal condition is destined to supply the various organs, is now lost by being converted into heat, a condition which will last until the original cause of these abnormal processes, the noxious poison, is eliminated from the organism. If this condition of things lasts only a limited time, no organic changes will take place in the nervous tissues, but, on the contrary, they will, with the subsidence of the fever and the accompanying exchanges of matter, regain their integrity, as happens in all cases of recovery from yellow fever, and also in most other cases of hyperæmia of the brain. In the more violent cases of yellow fever, however, in which the febrile process is prolonged and particularly stormy in character, the hyperæmia becomes persistent and manifests itself by violent delirium, convulsions, or other serious nervous phenomena, which have been regarded as depending upon the degenerated condition of the renal parenchyma, and the ensuing uræmia. That this is not the case, but that, according to the theory of Traube, they in reality depend upon an œdema, or any other diseased condition of the brain, deranging the normal functions of this organ, I have already sufficiently shown in the preceding pages, and can, moreover, corroborate by the œdema and anæmia of the brain

which I observed, two years ago, in a man who had been for a number of years an habitual opium-eater, and who died under severe convulsions, to which he had already been subject before his death. In a number of fatal cases of yellow fever, the brain, as I have shown, is found œdematous, which here, however, depends upon a hyperæmia, a condition, it is true, opposite to that which Traube assigns to the uræmic convulsions. This apparent contradiction, however, is easily explained by the fact that some of the accompanying phenomena of anæmia of the brain, such as convulsions, may likewise be produced by a hyperæmia of that organ. In both cases, the symptoms depend upon a disturbance of the normal nutrition of the organ, arising, in the one, from a diminution, and in the other from a derangement, or complete interruption, of this function.

The deranged nutrition of the nervous tissues, caused by a persisting hyperæmia, will also eventually give rise to degenerative changes of these tissues, or their minute blood vessels. Such changes may be observed, in a number of fatal cases, in the fatty degeneration of the minute blood vessels and of ganglion-cells of the sympathetic ganglia, and, even, to some extent, in those of the cortex cerebri. The degenerations which I observed in the semi-lunar and first thoracic ganglia, consisting in the disappearance of the nuclei of the ganglion cells, and in the fatty and atrophied appearance which the latter themselves presented, cannot but exert a most deleterious influence upon the organs which they supply, by greatly diminishing the nervous energy which they need for the preservation of their integrity. With the degeneration of the nuclei of the ganglion-cells, which I regard as special reservoirs, or stores, of potential energy, the influence which these nerve centers exert upon the nutrition of their respective organs, must become considerably diminished. The pathological changes in the kidneys, supra-renal bodies, and liver, therefore, may stand in a close relationship with those observed in the semi-lunar ganglion, and other ganglia of the solar plexus. In the same manner may the changes in the muscular tissue of the heart depend upon those in the ganglion stellatum. But, judging from these changes, found in those sympathetic ganglia to which my examinations extended, viz.,

the ganglion stellatum, ganglion semilunare, and some others of the solar plexus, we may well presume that they had equally taken place in a number of other sympathetic ganglia, especially those of the cardiac plexus; and if, furthermore considering that the nerve centers which regulate the action of the heart, as well as its nutrition, send their stimuli through the ganglion stellatum and cardiac plexus, it becomes obvious that these changes cannot but exert a depressive influence upon the action and nutrition of this organ.

Thus it happens that, while during the febrile stage of the disease the heart's action is accelerated by the irritative influence of the poison upon these centers and ganglia, it is depressed during the second, but particularly the third stage, when the nervous energy becomes abnormally diminished by these pathological changes and the ensuing exhaustion of the nerve centers. And if this nervous depression passes beyond certain limits, a collapse of the organism must evidently be the result; but it may also terminate in paralysis of the heart, causing the death of the patient. In very severe cases, the nervous depression is frequently associated with a degeneration of the muscular tissue of the heart, which is likewise cutting off every chance for recovery. Excessive nervous depression, or paresis of the heart is, therefore, one of the most dangerous symptoms in the whole course of the disease, especially when it appears in the last stage of the disease, as it may then be associated with the degeneration of the muscular element, and, if not soon relieved by stimulation, must evidently lead to collapse. The feebleness and rapidity of the pulse, together with the diminution of the peripheral heat, especially of the extremities, indicate the approaching danger, the action of the organ being too low to properly circulate the blood through the peripheral vessels. The muddy, cyanosed appearance of the face, shortly before death, also depends upon this cause.

In taking a final review of the nervous phenomena of yellow fever, we find then that, while in the commencement of the disease they depend upon a paresis of the vaso-motor nerves, this condition is soon followed by an irritation of the different nerve centers mentioned, situated in the medulla oblongata, and giving

rise to the temporary acceleration of the heart and the irregular contractions of the cutaneous blood vessels during the hot stage; but that, in the second and third stage, this abnormal excitation is relieved by a general depression and exhaustion of the nervous system, in many cases associated with paresis, or even paralysis of the heart. But though the latter condition, which mostly takes place in protracted cases, may be the immediate cause of death, the hyperæmia of the brain is not relieved, but persists in a more chronic form until the fatal issue, when its presence will be revealed by the autopsy. That in fatal cases, without regard to the immediate cause of death, the neuro-paralytic condition of the blood vessels persists until this event takes place, is corroborated by the larger cerebral arteries, even the basilar, being found filled with blood, which is not the case in other diseases in which death occurs from other causes and without hyperæmia of the brain, where the larger and smaller arteries contract during the agony, driving the blood through the capillaries into the veins.

The pathological changes, observed in the supra-renal bodies depend upon the same causes, and take place in the same manner as those in the kidney, liver, and other organs; but, for the reason that so little is known concerning the true function of these organs, I shall forbear making any remarks as to the part they may play in yellow fever.

In comparing the clinical phenomena and pathological changes of yellow fever with those of other infectious diseases, we find that they resemble each other in many points. Thus, in the typhoid diseases, such as typhus, typhoid, bilious typhoid, and relapsing fever especially, the same nervous disturbances, caused by the influence of the infectious poison and the ensuing derangement in the nutrition of the nervous tissues, are equally met with. The extravasations of hæmoglobin and the general tendency to capillary hæmorrhage, as well as other degenerative processes in the glandular organs are also observed in numerous cases of these diseases. But, notwithstanding the similarity existing in the above phenomena, there are some other important points, in which yellow fever essentially differs from these typhoid or other kindred diseases. The difference particularly concerns the so-

called "blood-making" organs, the spleen and the lymphatic glands, which in the above diseases are almost always more or less affected, while in yellow fever they remain perfectly normal. This circumstance also explains the fact, that in these infectious diseases the blood is frequently found to have lost its coagulability, that is, it has in the true sense of the word become contaminated, or prone to decomposition, which never, or, at least, *very rarely* takes place in yellow fever, though it is believed by a large number of physicians. In the latter, the infectious poison appears to exert its noxious influence especially upon the morphological constituents of the blood, the colored blood corpuscles, inducing them to part with their coloring matter, the agent in the processes of oxydation taking place in the organism; and the congestions occurring depend less upon a true contamination of the blood, than, as has been demonstrated, upon a paresis or paralysis of the vaso-motor nerves.

Another difference may be found in the duration of the febrile process, the extent of which seems to be typical, as regards its length of time, to each individual infectious disease.

Besides, many of the phenomena of yellow fever are also met with in those infectious diseases, known as "*exanthemata*," and in the different forms of miasmatic disease, so that only a few symptoms remain to be regarded as *pathognomonic* of this disease. This leads us to the question, How then, may yellow fever be distinguished from the above named diseases, and how may sporadic cases be correctly diagnosed during the absence of an epidemic?—questions quite difficult to answer. In the commencement of the disease, and, even, after the febrile process has started into action, it is almost impossible to make an infallible diagnosis, as a number of other infectious diseases commence in the same manner, accompanied by the same phenomena. In the diagnosis of yellow fever, therefore, *the whole complex of symptoms must be taken into consideration.* On the second day, the general observation concerning the relative state of the pulse and temperature, to which I have referred before, may be applied. This observation consists, as will be remembered, *in the falling of the pulse on the second day, with a simultaneous rise of the temperature of the body.* The presence of albumen in the urine on the

third day, also, may to a certain extent serve as a pathognomonic symptom. If the disease runs on to its third stage, and black vomit should appear, all doubts about the nature of the disease, of course, must vanish. In fatal cases, the autopsy must decide the question from the pathological changes met with in the different organs, for *fatty infiltration of the liver, associated with that peculiar congestion of the stomach, together with black vomit*, are pathognomonic phenomena of yellow fever.

As regards the prognosis of the disease, the reader may draw his own conclusions from what has been said in the preceding part of this treatise.

[TO BE CONTINUED.]

ARTICLE V.

EXTRA UTERINE FŒTATION. By H. ROZENCRANS, M.D., Elgin, Ill.

On the 28th day of July, 1880, I was called twenty miles into the country to see a sick woman, a Mrs. J., aged 28 years, the mother of two living children. I found this woman suffering severely, and was informed that for three nights she had not slept. Her sufferings were referred mostly to the lower part of her back. I administered to her tincture of opium, and bromide of potassium, a liberal dose; this caused her to sleep most of the night. Examination of this woman satisfied me that she was a subject for an operation; that it would not admit of delay, as she was then very much emaciated; could not retain food, could not rest night nor day, because of great suffering. I advised her to be put on an easy spring wagon, on a mattress, and carefully brought to town for treatment. This was done. I then made a thorough examination of the case, and diagnosticated it a case of extra-uterine fœtation, and from the history of the case to be of about one year duration.

As there was evidently a considerable quantity of fluid in the abdominal cavity, and to satisfy myself more fully of the correctness of my diagnosis, I resolved to introduce the trochar, and did

so, about one inch below the navel, on the median line, and drew off at least two gallons of a dark heavy fluid. I then made further examination and could distinctly feel a solid substance in the abdominal cavity and pronounced it a fœtus. I wrote down my diagnosis, and sent it to a physician living in a neighboring town (Victoria), fifty miles away, inviting him and two others to come down and assist me in a operation for the removal of an extra uterine fœtus by abdominal section. Dr. Sutherland and Dr. Blake, the former of Victoria and the latter of Cueso, finally came down and assisted me in the operation, on the 20th day of August, 1880, in Indianola. I, after getting this patient in readiness, administered to her chloroform, and immediately cut down through the abdominal walls on the median line extending from one inch below the umbilicus to near the pubic arch. After cutting through the walls of the abdomen, bringing to view the cyst, I had the patient turned over on her side and at once cut open the cyst, from which flowed freely a considerable quantity of the same character of fluid that I had removed a few days previously with the use of the trochar. I then at once thrust my hand into the sac and grappled the fœtus and at once removed it, feet foremost, dead, and weighing eight pounds. I then tore away the placental mass, and proceeded to the removal of the sac. This I found a difficult thing to do because of its numerous attachments; portions of it I cut away, portions of it I tore away; a portion of the omentum was removed with it. The right ovary was removed, after ligaturing the pedicle with a cat-gut ligature. The wound was then closed, leaving an opening at its most dependent part, and a tube introdubed for drainage, from whence for many days a copious discharge of putrid matter flowed. To obviate internal bleeding I, immediately after closing the wound, placed two bricks on the abdomen for pressure, leaving them there for half an hour and then removing them. During the operation I used the carbolic spray and diligently, during the after treatment, kept injecting daily into the wound, through the tube, a solution of five per cent. of carbolic acid. The weather was very warm, averaging 95°. I applied pounded ice constantly for the space of three weeks to the abdomen, and for that length of

time gave a quinine pill three times a day, of two grs. each. Ordered a milk diet. I drew off her urine twice a day for a week, after that she voided it herself. Her first evacuation from the bowels was on the fifth day. After the operation I never gave her any cathartic medicine. She never suffered any pain after the operation; the temperature of her body remained quite normal, her pulse was usually one hundred in the morning and would rise to one hundred and twenty in the after part of the day, for about three weeks. I consider that the constant application of ice prevented peritonitis, and with the diligent use of carbolic acid prevented pyæmia or blood poisoning. I discharged my patient cured, on the 6th day of November, seventy-eight days after the operation. This operation was performed on the 20th day of August, 1880, in the Town of Indianola, State of Texas. I was one hour and a half in performing it. I am not aware that a similar operation was ever before performed in that State

June, 1881.

ARTICLE VI.

THE COURSE OF THE BULLET IN THE PRESIDENT'S WOUND.

By EDMUND ANDREWS, A.M., M.D., Professor of Clinical Surgery in the Chicago Medical College.

Having recently conversed with Prof. Frank H. Hamilton and others, who have specially investigated President Garfield's wound, I think a few of the facts elicited may interest the profession.

The symptoms are such as make it a matter of doubt what the real internal injuries are. The would-be assassin shot him from behind, but the exact direction of the pistol at the moment of discharge is not clearly known. The attending surgeons agree that the bullet entered the back four inches to the right of the center of the spine, and traversed downward and forward through the mass of spinal muscles, and that it struck the right eleventh rib, and thence passed inside the bony walls of the thorax. The bul-

let was a conical one of the caliber $\frac{44}{100}$, and weighed 200 grains. The point was truncated, probably to make the cartridge short enough to be used in a short cylinder. This fact, and the additional one, that the charge of powder was light, gives increased probability that in striking the rib its course might be deflected.

The attending surgeons very properly judged that vigorous probing was unsafe. However, Surgeon-General Wales, of the Navy, introduced his little finger, and thought he felt the liver, and that the upper edge of the eleventh rib was notched or injured. Immediately after the shot the President felt pain in the distribution of the right great sciatic nerve, followed not long after by pain felt equally in the distribution of the great sciatics of both limbs. It is not quite easy to account for this fact. A probe is said to have been introduced three and a half inches by another attendant; but every one knows that probes in such cases run into intermuscular spaces with surprising facility, and give very imperfect information. In addition to the pain in the legs, the President felt a sense of heat and deranged sensation in the skin of the right inner part of the thigh, and right side of the scrotum.

The patient vomited repeatedly, but threw up no blood, nor was there any stain of blood in the sputa, alvine discharges, nor in the urine. There was some retention of urine, relieved by the catheter. There was some, but not excessive, shock, followed by a fair, moderate reaction.

The surgeons at Washington believed the liver to be wounded, and judged it probable that the bullet had passed through that organ, and lodged in the anterior wall of the abdomen, or in the groin, possibly wounding intestines on the way. As general peritonitis did not supervene, the fear respecting the intestines subsided, together with the anxiety about hæmorrhage. A spot in the right hypochondrium showed an ecchymosis and tenderness and was thought to be the possible location of the ball.

Now what was the nature of the President's wound? As to the liver having received the bullet, the evidence seems only that the location of the wound rendered it probable. Dr. Wales thought he felt it with his little finger, but liver tissue is not so characteristic to the touch as to secure him against mistake,

unless he explored with an energy which would be dangerous and unjustifiable. If the bullet notched the upper edge of the eleventh rib, and thence entered the liver, it must have passed across the lower edge of the posterior part of the right lung, wounding that organ, opening the pleura, and piercing the diaphragm. In such case Dr. Wales would have to pass his finger through the pleural cavity to reach the liver. If this were the course, the patient should have spit blood, and had an escape of air, followed by pleuritis, and at least some pneumonia, neither of which has occurred. If the bullet was deflected downward by the tissues which resisted it, and by striking the eleventh rib, so as to escape the pleura and the lung, such a course would be likely to carry it behind the liver and not into it. I understand Dr. Hamilton that Dr. Wales' examination with the finger gave him the impression that only the upper border of the rib was notched. If this were so, the glancing of the bullet would throw it more forward instead of downward, and the wounding of the lung and the liver would be sure.

As the lung certainly escaped, and as there is not a single clear proof of the liver being touched, it seems necessary to suppose that Dr. Wales, examining, as he doubtless did, briefly, was not able to get an accurate idea of the condition. A splinter of rib driven downward might give him the impression of the upper border of the bone. It is also conceivable that a hasty examination of a somewhat stout man like the President might cause him to mistake the twelfth rib for the eleventh. At any rate there has not been a single decisive symptom of a wound of the liver, so far as I have been able to learn, though plenty of reason to fear its possibility.

Prof. F. D. Weisse, of New York, after experiments and dissections upon the cadaver, authorized the following report in the *N. Y. Herald:*

TO THE MEDICAL PROFESSION AND THE PUBLIC: In view of the fact that I have been reported as having for several days past been engaged in making some investigations bearing upon the probable nature of the pistol shot wound inflicted upon the President, and because the published statements are disjointed and vague, I desire to make this public statement of my theory and observations relative to the case. On the second day after the shooting of the President it seemed possible that the wound did not

involve the peritoneum, digestive canal or right kidney, and if so the track and lodgment of the ball were yet unsolved. Studying carefully the official bulletins and the statement of the physicians in attendance it seemed o me that the weight of evidence pointed to an injury of the sacral plexus of nerves. I determined to investigate the subject, hoping to be able to contribute a possible clew to the unravelling of the case. I conceived the theory that by some fortuitous circumstance the ball had been deflected from its course by striking and fracturing the eleventh rib; that it had passed along the anterior surface of the transversalis muscle that forms the interior muscle wall of the outer half of the loin, passing between the transversalis muscle posteriorly and the kidney and ascending portion of the large intestine anteriorly; thence into the iliac fossa (which is the bed of the right groin), between the iliacus internus muscle posteriorly, and the cæcum or commencement of the large intestine anteriorly; thence continuing downward and inward through or behind the psoas magnus muscle to the right side of the pelvis (or lower cavity of abdomen), there inflicting an injury to one of the nerve trunks of the sacral plexus itself, where the ball is now probably lodged. This being the supposed track of the ball it would plow through a mass of fat which exists between the muscle walls behind and the abdominal organs above mentioned in front. This layer of fat in President Garfield (judging from his physique as pictured and described), is probably from an inch and a half to two inches thick. Two nerves cross the supposed track of the ball, lying upon the anterior surface of the transversalis muscle between it and the fat—namely, the ilio-hypogastric and ilio-inguinal branches of the lumbar plexus. These two nerves pass round each side between the muscle planes of the abdominal wall, reaching the groin, where they emerge at the side of the median line at the lowermost point of the belly, distributing nerve films to the skin of that region and to the skin of the scrotum, which areas of skin they endow with sensibility.

The symptoms which seemed to lead to this theory were, first, that the President fell on the receipt of the wound without losing consciousness; second, that he vomited almost immediately after falling; third, that upon being asked immediately after the receipt of the wound if he had any pain, he answered, "Yes, in my right leg and foot;" fourth, the occurrence of tingling and burning of the skin of the legs and feet together with the excruciating pains and cramps of the same; fifth, that upon the cessation of these pains he was left with a feeling of soreness of the muscles and exaggerated sensibility of the feet and legs. To test the theory of the deflection of the ball and its course I investigated the qualities of the weapon that inflicted the wound and made special dissections of the region involved. As regards the pistol used I hope to find some inherent defects in it which might tend to bear out the probability that the ball fired from such a pistol could be deflected; I had even thought that if it did prove a reliable weapon; that the particular cartridge fired might have been a poor one. I obtained a British bull-dog pistol and a box of forty-four caliber cartridges, and upon inquiry was informed that as a weapon it was defective in many particulars, both as to pistol and cartridge; first, the cartridge

contains eighteen grains of powder in a very shallow cup, the ball weighing 200 grains; second, that the ball is cut off at the end and not pointed; third, that the shortness of the barrel causes a loss of the effectiveness of the powder at the muzzle in firing; fourth, that as a weapon of precision in firing it is defective in the feeble grip afforded by the small and awkward shape of the handle; also, that the shortness of the barrel tends to elevate the muzzle at the time of the explosion of the cartridge. Upon testing the effectiveness of the weapon I found first upon firing at a one inch board ten feet off that the ball went through the board, but did not have force enough to penetrate a second board eight inches beyond and dropped to the ground; second, firing into the trunk of a suspended cadaver I found whenever the ball struck a flat bone it would lodge in the body; if it did not impinge upon a flat bone of the chest it would at times go straight through, but more often would lodge at an opposite point beneath the skin. Firing into the abdomen it would lodge in the body, owing, probably, to the mobility of the organ. The above observations would seem to show the possibility of a ready deflection in the ball.

The ball that entered the President's body is described as having entered four inches to the right of the median line of the back, fracturing the eleventh rib. The eleventh is the most movable of all the ribs, being free at its anterior end and not steadied, as is the twelfth rib, by a muscle attached to its lower edge. The conditions favorable to reflection by impingement upon and fracture of this rib are—first, the mobility of the rib makes it like a hickory twig fixed at one end only, and it swaying upon impact would expend a good deal of the force of the ball; second, the fracture of the rib would contribute still further to break the force of the ball; third, the external surface of the rib being convex tends to deflect a ball (of all bones of the body the rib probably deflects balls most often); fourth, if the eleventh rib is pushed inward from behind it rises anteriorly, and if a ball struck it, it would tend to turn the ball on its axis and deflect it downward and inward; fifth, the clothing of the President must also be considered as an element contributing to diminishing the penetrating power of the ball before it reached the rib, but I have no data with reference to the clothing. The above conditions at the point of entrance of the ball as impinging upon and fracturing the movable eleventh rib seem to have afforded contributive elements to facilitate deflection of the ball downward and forward.

I obtained a cadaver six feet high, but the body not quite as stout as that of the President, suspended it so that the feet rested on the floor, making it assume as near as possible the position in which the President stood when shot. A twelve inch trochar (a steel rod one-quarter inch in diameter, pointed at the end and fixed in a handle), was entered exactly at the point of the President's wound and made to pin all the tissues and organs together, so as to steady them during the progress of dissection. A careful dissection of the right loin was then made, layer by layer, from the skin to the internal organs, from an area extending from the tenth rib above to the crest of the ilium below, and from the median line flaps of the layers were turned off to the side. In the track of the wound the skin, sub-cutaneous tissue, the latissimus dorsi muscle plane and that of the serratus posticus inferior

muscle were removed, exposing the tenth, eleventh and twelfth ribs and their intervening muscles; also the exterior surface of the transversalis muscle for the external half of the loin and the erector spinæ muscle for the inner half of the loin. The external intercostal muscles were dissected out of the intercostal spaces and the intercostal vessels and nerves recognized, as also the costal layer of the pleura bridging the inner portion of the spaces to line the interior surfaces of the rib; in the external portion of the intercostal spaces; after the removal of the external intercostals, the posterior position of the internal intercostal plane of muscle was presented. The costal layer of the pleura was separated from the eleventh rib and one inch of the rib corresponding to the probable point of fracture was cut out with bone forceps. The pleural cavity was opened, and, the finger being inserted, found a pocket extending down to the twelfth rib, as the pleural attachment rises posteriorly toward the spine and anteriorly toward the eleventh rib. Three planes of tissue covering the convexity of the liver were now brought into view and were presented in the following order:—First, the diaphragmatic layer of the pleura; second, dissecting this off exposed the muscle structure of the diaphragm (the diaphragm being attached to the eleventh and twelfth ribs); third, the peritoneal lining of the abdominal surface of the diaphragm. Cutting through the peritoneum brought into view the thick lateral superior border of the right lobe of the liver. Removing the diaphragm a little lower down there was exposed the external and upper border of the kidney, there being no peritoneum intervening.

A large director was slipped in anterior to the tip of the twelfth rib in a direction downward on to the internal surface of the transversalis muscle. It passed readily downward and inward, following the course of the loin into the iliac fossa. The twelfth rib was then cut through and dissected out with the bone forceps, carrying away its diaphragmatic attachment; the transversalis muscle was sectioned along the director, down to the crest of the ilium. The ilio-hypogastric and ilio-inguinal nerves were dissected out and preserved *in situ*. The hand was inserted, crowding the fat behind the kidney, with the large intestine (the peritoneal sac being intact) forward; the fingers passed with great facility downward and inward into the iliac fossa until the palmar surface of their tips rested on the iliacus internus muscle, the nails against the external and inferior surface of the psoas magnus muscle (this particular point is situated at the back and internal bed of the groin.) The fingers, when crowded a little, passed to the surface of the anterior crural and obturator nerves of the lumbar plexus; crowding them still further the lumbo-sacral cord of the sacral plexus and even the sacral plexus itself could be felt. The peritoneum was now opened anterior to the external convex border of the kidney, exposing the external lateral border of the right lobe of the liver. The fat from the kidney was removed, and the organ was recognized as bedded in the fossa in the posterior and inferior surfaces of the right lobe of the liver. A ball to reach the external border of the right lobe of the liver would have to enter the cavity of the peritoneum and then penetrate the liver.

The dissection being now completed and the relation of the organ

involved in the region of the liver being rendered appreciable, a careful drawing, by Mr. Max Cohn, of the region was made, which, upon subsequent comparison, was found to accord exactly with the illustration of the region made by our standard authorities. The dissection of the region thus seemed to give additional possibility to the theory of the course of the ball and its present lodgment.

On the morning of Thursday, July 7, I called upon Professor Frank H. Hamilton, M.D., and stated to him my theory of the probable deflection and course of the ball, giving him my reasons therefor and my investigations and the data I had obtained relative to the bullet and the pistol and the results of my dissections of the injured region. I then asked the doctor to give me a detailed statement of all the facts in the case that he had learned at the time of his visit to Washington as one of the consulting surgeons. This he kindly gave me, and among other symptoms stated that the President had called attention to a peculiar sensitiveness of the skin of the right side of the scrotum. An injury to the ilio-hypogastric and ilio-inguinal nerves, which lie in the supposed track of the ball, according to the theory advanced would account for this peculiar sensation. This, indeed, seemed to be a most happy confirmation of the probable correctness of the theory. Encouraged by Professor Hamilton I repeated my dissections on July 7, and he made an appointment to be present at a demonstration of the same on the following day. On July 8 I obtained several cadavers resembling the physique of the President, and at two P. M., in the presence of Dr. Hamilton, Dr. George F. Shrady, editor of the New York *Medical Record*, and other professional gentlemen, I repeated the dissections, pistol firings, etc. I propose to continue these dissections, and carry them still further, in order, if possible, to determine the probabilities as to how eventually the ball will be dislodged and where it will most likely point, if, perchance, it is where I think it likely to be. FANEUIL D. WEISSE,

Professor of Practical and Surgical Anatomy in the Medical Department of the University Medical College of New York.

The Professor having telegraphed July 8, to the Union Metallic Cartridge Company, Bridgeport, Conn., for data, with reference to the 44 cartridge, received in answer the following telegram:

UNION METALLIC CARTRIDGE COMPANY,
BRIDGEPORT, CONN., July 9, 1881.

In reference to the 44 central fire cartridge, would say that the exact amount of powder contained in the cartridge is twenty grains; weight of bullet 200 grains. The crimping or turning in of the shell in the end makes the cartridge shoot stronger. C. L. RICHMOND.

The *Herald* also contains an interview with Prof. Frank H. Hamilton, who has been at Washington in consultation on the President's case, and who expressed the opinion that there is no conclusive proof of a wound of the liver.

Dr. Weisse makes it highly probable that the bullet glanced downward behind the liver without touching it, but the further

course of it as marked out in his theory seems very improbable. That the shot should enter the abdomen through the eleventh rib, slide down to the pelvis, and curve across the iliac fossa without wounding the colon, kidney, ureter, anterior crural nerve, nor either of the iliac arteries, and then, turning abruptly backward into the hollow of the sacrum, wound neither the bladder nor rectum, but injure there the roots of both the right and left sciatic nerves, so as to cause the pain in both feet and legs, seems, though not absolutely impossible, very difficult to believe. The supposed lodgment in the hollow of the sacrum without wounding arteries or viscera, and the injuring the sacral nerves so as to cause pain without paralyzing motion is highly improbable, for the motor and sensory fibers there run mingled in the same trunks, and would be alike injured. It is more probable that when the eleventh rib was shattered the corresponding costal nerve received a violent jerk, which affected the adjacent cord mechanically where the sensory tract is sufficiently separate from the motor columns to receive an isolated injury. This would account for the sensory disturbance of the feet and legs better than a wound of the sacral plexus. If the liver has been really wounded it would not necessarily be fatal. The statistics of the late war, published by the War Department, may be condensed as follows:

Uncomplicated gunshot wounds of the liver—Cases, 59; deaths, 34; mortality 58 per cent.

Gunshot wounds of liver, complicated with other injuries—Cases, 114; deaths, 74; mortality, 66 per cent.

The army statistics have very little value for determining the President's risk, because the correct diagnosis was frequently impossible, and the projectile used in the regulation musket was much larger than the one fired by Guiteau, still they show that even if the liver is really wounded it will not necessarily be a fatal case. As above stated, I consider the present location of the projectile entirely undetermined, but that the probabilities are against it being either in the liver, or in the hollow of the sacrum.

No. 6, Sixteenth Street, Chicago.

ARTICLE VII.

CREMATION. By Dr. C. W. PURDY, of Chicago.

Death made its advent into our world through the agency of a dreadful crime, and ever since Cain thought to conceal the body of his victim with earth (the first burial) has the disposal of the dead been an anxious question with the living.

Many contradictory and superstitious customs of disposing of the dead were in vogue among ancient nations and barbarous tribes, but these are of little use to us as precedents. Herodotus, Cicero and Lucian inform us that some Asiatic nations feasted on the slain, and murdered the sick and aged; that others threw their dead to ferocious animals; that others cast them into seas, lakes and rivers; that the Scythians buried them in the snow. According to Spondanus, the "Syrcanians flung their dead to the dogs; some Indians left them to vultures; the Celts took from them the vertex of the cranium, which they set in gold for goblets." But as man became more civilized, the voice of religion, reason and policy called for the careful interment of the dead.

The Egyptians attached to public burial an idea of the greatest honor, as indeed did also the Greeks and Romans, and it was considered the highest recompense of virtue, and hence the desire of becoming worthy of funeral obsequies was universally cherished. It was a crime to disturb the repose of the dead in their last abode. A reverence for tombs thus became a part of religious worship, and to pay the last duties to the dead was held a sacred obligation.

Man, impelled by his passions, always oversteps the bounds of reason. The affectionate zeal of afflicted relatives invented at length the art of giving a kind of perpetuity to the inanimate form, and thus the early Egyptians embalmed the dead; using every art capable of preventing the action of the air upon the body to prevent corruption, and to secure it in such a manner, that it might be kept without danger to the living.

Self love gave much encouragement to this art. It was

believed that as long as the body remained entire, the soul hovered near it. Thus they were kept as a most precious family deposit, and a sacred civil pledge. The inevitable result followed of delaying Nature's processes. Contagious diseases broke out in the midst of the Egyptians and baffled every remedy; and all the embalmed were obliged to be removed to a distance. The great number of dead, after the carnage of battles, obliged them to resort to burning. In time the old practice was completely changed; and tombs and vases were only used for the ashes of the funeral pile.

Long wars, frequent transmigrations, ruin and rebuilding of cities in time overturned whole countries, and bones confided to dust for centuries were exposed and disturbed unavoidably. As this was a profanation nothing short of the most awful sacrilege, it resulted at length in the general determination to adopt burning of the dead. They even excluded the venerated ashes from within the walls of cities; and deposited the urns in places consecrated to burial. The highways were for a long time bordered with tombs and slabs of marble covered with inscriptions.

"One consequence of stationing tombs upon the roads beyond the gates was that towns attacked were not as liable to slaughter and destruction; the citizens would leave their walls for the defense of those sacred remains."

We will find the elements of our own customs by a glance at the rites practiced by the Hebrews, Greeks and Romans.

Inhumation was practiced perhaps more universally by the Hebrew than any other race, and hence the origin of the Christian burial; and yet this is the only Jewish custom retained by them. Abraham bought from the children of Heth the cave of Hebron and deposited there the corpse of Sarah. He himself was also buried there, as was Isaac, Rebekah and Leah. It is believed, as commentators declare, that the remains of all the illustrious patriarchs were assembled in the cave of Hebron with the bones of Abraham. Vaults dug in the hill of Zion, under the foundations of the temple, and in the royal gardens, were destined for the last abodes of the kings of Judah.

The course of time brought but slight changes in the practices of this people, although they underwent such eventful vicissitudes.

Although burial was the chief method of disposing of the dead, it was not the exclusive one. Among the great, especially kings, some secured the honor of embalming, and cremation was occasionally adopted for the same purpose (Jer. xxxiv. 5); probably this custom was of short duration and only peculiar to a few.

The bodies of Saul and Jonathan, however, were burnt to ashes by the people of Jabesh-Gilead in order to rescue them from the insults of the Philistines. We are also told that "a continual fire that consumed the carcasses and the filth of the city, burned always in the deep pit of Topheth in the valley of the children of Hinnom. (Isaiah xxx. 33.)

To the Greeks belong the credit of more uniform practice of burning the dead than any other nation of ancient times. They inclosed the urns which contained the ashes of their dead in private houses, within cities, and sometimes in their temples. Some traced back the origin of the custom to Hercules, who wished to carry to King Licinius the last remains of his son Argivus who perished in battle.

In the history of the Roman nation, nothing is perhaps more interesting than the endless variety of laws, rites and forms which they observed toward the dead. One half of the ruins or emblematical antiquities remaining, relates to this.

Numa Pompillius was buried on the Janiculine Hill; the succeeding kings were buried in the Campus Martius, a large plain along the Tiber. Ancient Rome permitted none of her citizens to be buried within her walls, save the vestal virgins. In course of later times, however, great heroes were allowed the same honor.

"During the many wars of the Republic and the incursions of the Barbarians, the impossibility of interring all human remains left exposed, necessitated their disposal by burning. Again, experience had taught them that they could not with safety bury their dead among themselves and their dwellings. To protect the remains of their venerated chiefs or companions in arms, the funeral pyre was adopted in imitation of the Greeks. The ashes were carefully collected by the friends of the deceased, enclosed in an urn and deposited in a monument, or in the catacombs, some of which still exist." (Pascalis.)

Thus we observe that three principal methods of disposing of the dead have been practiced: viz., inhumation, embalming, and incremation. The first originated with the first family of man, and has been more or less in practice to the present time. The second, embalming, if not originating with, at least was most extensively practiced by the Egyptians, and the perfection of this art attained by them has long since been lost to themselves and to the world.

Cremation of the dead was in more or less general practice among all the ancient nations of the world, but more largely so by the Greeks and Romans. Of the origin of this practice we are not reliably informed by historians. It is more than probable that it arose through a desire to preserve the dead from the passions and fury of the living, in those ages when man's barbarous passions impelled him to pursue his victims after death. Witness the fierce Achilles tying the body of Hector by the heels to his chariot, and driving around the city of Troy.

But the general adoption of cremation by the Egyptians, Greeks and Romans, was always the result of necessity for the protection of the living. About the end of the fourth Christian century its practice was discontinued, owing to the growing power of Christianity, whose doctrine of the resurrection, with its strong attachment to inhumation, brought a great influence to bear against it.

One of the most solid arguments in favor of cremation is from a sanitary standpoint; and this is what interests us more especially as medical men.

From medical societies should spring the germs of sanitary excellence, and it is eminently proper therefore, that we should give careful consideration to matters looking to so important a sanitary reform.

To appreciate more fully the sanitary advantages of cremation, let us first look at the ghastly record of our present burial system.

"The history of vaults, grave-yards and cemeteries is one long tale of desecration, robbery and outrage; coffins have been rifled from time immemorial for treasures; there is scarcely a museum in the world which does not bear ample witness of this

practice of spoliation; even professional grave-diggers have dug up the bodies of the high born for the purpose of finding rings and other treasures buried with them."

The London *Quarterly Review* (No. 42, p. 380), says "that many tons of human bones every year are sent from London to the north, where they are crushed in mills and used as manure."

It is impossible to preserve the dead by burial from the outrages of the living. Grave-diggers are often employed to remove bones; as a rule they are not scrupulous and are often drunk.

"Where are the thousands who were buried in the heart of Paris, and slept there for centuries in the tombs of the Innocents? You may to-day gaze on their bones and comment over their skulls. By order of the Minister of Police they were all dug up in 1707 and carried off to the catacombs. The bones were cleaned and arranged in the galleries; in one the arms, legs and thighs, intersected by rows of skulls; the small bones thrown in heaps behind. 'They were bourn there, we are told, by priests and tapirs!'" (Hawes.)

A New York correspondent affirms "that a number of men were recently employed as grave-diggers in one of the old graveyards of that city. Bodies were offered for sale on the ground to a party of medical students, who shrank from the horrors they witnessed. One coffin was found to contain a heavy decomposed mass like spermaceti; it was used to grease the axle-tree of a cart. Another coffin contained the body of a woman aged twenty, as the inscription announced. She had rested for 100 years—laid there with what tears, what tender regrets of husband, brother or mother! but now her head was rudely seized and kicked like a foot-ball from one ruffian to another."

Previous to the passage of the Burial Acts, 1852–56, violation of graves in Scotland, England and Ireland was systematic. "In most crowded burial grounds there was a bone-house; and, in many, a burning ground for coffin wood."

You paid for your grave; your friends thought you would lie undisturbed. If you were in a vault, your coffin might still be there; but after a certain number of years not your bones. Your bones would be in the bone-house, or sold for manure; your coffin burnt. In many parts of London coffin-wood was

used habitually for fire-wood, and coffin furniture (second-hand plates, nails, etc.,) was a well-known article with marine store dealers." (See Walker, "Gatherings from Grave-yards.")

A surgeon visited the burial-ground in Portugal street, April 27, 1839. He found two graves open, several bones lying about; and heaps of coffin wood lying about, some quite fresh, awaiting removal for firewood.

A gentleman writes to the London *Times*, June 25, 1838, of this same grave-yard: "I was shocked to see two men employed in carrying baskets of human bones to the back of the ground, through a small gate. I have twelve of my nearest relatives consigned to the grave in that ground."

A writer in the *Weekly Dispatch*, Sept. 30, 1838, says of St. Giles church-yard: "It is full of coffins up to the surface. Coffins are broken up before they are decayed and bodies removed to the bone house before they are sufficiently decayed to make their removal decent. This bone house is a large round pit; into this had been slid from a wheel-barrow, the but partly decayed inmates of the smashed coffins. On the north side was a man digging a grave. He was quite drunk."

This over-crowding of cemeteries and burial-grounds is one of the most dangerous abuses to which the system of burial is subject, and this is bound to occur in every grave-yard situated in populous districts. It only remains a question of time.

St. Giles and St. Pancras, of which we have been speaking, are said to be literally saturated with corpses; yet long after such was the case, the hearses streamed in to deposit their ghastly freight of human merchandise.

Look at most large towns—once a rural village, to-day a crowded city with a pest-house in its center. Such in time is the history of most burying-grounds. Look at those hundreds of plague acres closed by Acts of Parliament in England, and think what overcrowding did there.

Nor need we go so far from home for examples in this matter; look at the overcrowded condition of the cemetery of Trinity Church, New York, and on the malignant fever induced by overcrowding there in 1822, for an account of which see *Commercial Advertiser*, Aug. 7, 1822. Part of this same cemetery is now

walled up with its contained bodies, and the traffic of Broadway resounds above the once consecrated ground.

It is only a question of time when our own beautiful Graceland, Calvary and Rose Hill will have been overcrowded, and perhaps swept away by the resistless march of our city's marvelous and unprecedented growth. Already the voice of mirth and the prattle of our children may be heard any day over the sod of what, only thirty years ago, was our principal cemetery. Only a few years ago, and each day the dark caravans might be seen wending their solemn way northward to deposit the dead. In the same spot now is our most frequented and popular of public parks.

Where are the bones of those laid to rest there by our early settlers, and the founders of our city's greatness? Some have been carted away for a short time farther north, while some—many indeed—still lie beneath those public walks and driveways, while the wheels of revelry, and the tread of pleasure seekers sounds constantly over their remains.

"The church-yard sentiment is a false one, because it is untrue to facts. You think of the body in its quiet, hermetically sealed tomb; but time knows no such seal. The protecting slab, the solemn walls about the consecrated ground, are all changed to-morrow. The church-yard sown with flowers, its groves of drooping willows, the quiet, peaceful dead,—it is all a fancy. The voice of history tells us that in the past there has been no such seclusion, and what is to change the course of events in the future. The world is the world of the living, and attempts to make it a dead-house will break down in the future as it has in the past."

From the New York *Commercial Advertiser*, August 17, 1822, see the following: "It is not long indeed since we have seen, in this populous city, the mangled remains of the dead transported from one burying-ground to another. We have seen two church-yards opened, and deeply broken over an extensive surface, leaving exposed to our reluctant curiosity shattered limbs of corpses, and the decayed coffins, until they were closed by a range of brick vaults.

"These extend now a great way into the streets, literally under the pressure of a thousand horses, carts and carriages. An earthquake might in an instant throw up from under our feet the stings of death, the poison of human putrefaction, and create unexpectedly horrid scenes and cruel contrasts. It is well known that our large cities have, within a few years, extended much beyond their former suburbs; it has been necessary to build in many instances on what was formerly a burying-ground,—to expel, as it were, the dead for the accommodation of the living."

The over-crowding of grave-yards has led to a system of what grave-diggers call "management"; this is, simply making room for more bodies, and in some of the cemeteries has been carried to a disgraceful extent.

"The vaults of Enon Chapel and Clement's Lane are 59 ft. 3 in. by 28 ft. 8 in., allowing an average of 9 ft. per person. 200 would quite fill the whole area; fill that area six times, then the coffins lie in tiers six deep, and the whole space could not possibly contain more than 1200, and yet instead of 1200, between 10,000 and 12,000 were crowded into those vaults at various times! What became of them? They could not all rest there, thousands of them had to be managed." (Hawes, p. 27 & 28.)

The burial-ground of St. Cuthbert is situated in the center of the city of Edinburgh. (We quote from the *Scotsman*, March, 1874.) In the last fifteen years, 10,800 bodies have been placed there. Graves were re-opened up to 1874, some every seven years, some every three years; coffins with no plates were usually broken open, the remains were often not ripe (decayed), the coffin wood was burned, the remains heaped back pell mell upon others, to make room for more. At last but one or two inches of earth could be placed between the coffins. When all this came out in court the sheriff's address amounted to a defense of St. Cuthbert's."

The following is quoted as a notorious fact in the current prospectus of the Woking Necropolis Co. of London, 1874:

"In 1850 the Board of Health condemned the cemeteries of Highgate, Kensal Green, Norwood, Nunhead, and Brompton, and declared that they must be closed in the interests of public health. A central burying-ground was to be purchased at Abbey Wood, and Parliament was to buy up the old cemeteries. It bought

Brompton only, and closed it—no, used it, and is using it now. The government is accordingly using at high pressure and remunerative rates, a grave-yard condemned by its own board over a quarter of a century ago."

The necessary limits of this paper will not permit our dwelling at greater length on this over-crowding system of burying the dead. Those who wish to investigate the extent to which it has been practiced, and the sickening results, we would refer to a work published by Mr. Walker, entitled "Gatherings from Graveyards."

Mankind, until late years, have not devoted much consideration to the investigation of causes which might so far corrupt the atmosphere, as to create mortal diseases during marked periods of time; or why these plagues should appear in the vicinity of stagnant pools, shallow waters, receptacles of dead animals, or vegetable substances, in the neighborhood of slain after battle, in cities ravaged by famine; why columns of air have been rendered so noxious that the very birds that attempted to fly through them fell and perished; why the winds disseminating these aërial poisons, transported the breath of pestilence into rich, fertile, and thickly inhabited districts.

With the story of such terrible devastations the pages of history are filled. Putrid and malignant fevers, and periodical diseases often make their appearance in populous cities, without any apparent cause. May not this cause, known to us principally by its fatal results, be the practice of interment in the very midst of our dwellings? Epidemics which have laid waste whole cities have originated in the practice of burying in temples and churches. Diodorus, of Sicily, speaks of pestilences that were produced by the putrefaction of animal substances. Egypt is ravaged almost every year by malignant fevers, and from that country small-pox has spread over all the earth. The waters of the Nile leave with their slime an immense number of aquatic insects, which, as they corrupt, fill the air with pestilential miasmata. Lucan speaks of the ravages of an epidemic in the army of Pompey, near Durazzo; this was caused by the carcasses of horses, killed and left upon the field. Ammienus Marcellius informs us that the camp of Constantine the Great, was desolated

in consequence of the same imprudence. Frequently the corpses scattered over the field after battle have been the source and origin of disease and death on a large scale. The war of the Swedes in the seventeenth century, occasioned in like manner the great pestilence of Poland. Long and obstinate wars had the same effect in Austria, Spain, and in many other countries.

As we write at present, a plague is devastating Nedjeff, in Mesopotamia. "In this place is the grave of Ali, son-in-law of the prophet Mahomet. To this place his followers send the dead bodies of their friends and relatives, because they believe to be buried near this spot will assure their souls admission to Paradise. Caravan after caravan arrive there daily and deposit their ghastly freight for interment. The whole country about Nedjeff has become one vast grave-yard. The frequent floods occurring in the Euphrates, inundate all the land on both sides of the river. The light covering of earth is swept from the coffins, they fall to pieces, and thousands upon thousands of corpses are left rotting under the oriental sun, filling the air with their pestilential odors." Is it any wonder that a correspondent writes, May 2, 1881, that during the week just ended an average of 56 persons died daily out of a population of 6,000.

Both animal and vegetable substances, if subjected to heat and moisture at the temperature of 72° F. are converted in a little time into a variety of gases, carbonous, carbonic acid, carburretted, sulphurretted, phosphoretted, hydrogenous, etc., with azote, all of which are deleterious and deadly in their effects when inhaled into the system. It follows that carcasses can not be long exposed to heated air without becoming hurtful to persons in the vicinity of them. It has been argued, that provided a thick or heavy layer of earth be interposed between the dead material and the external air, no such gases can reach us to do any injury, and that they cannot remain long in their specific nature, but must soon be absorbed or decomposed and destroyed. This argument is not in keeping with the laws of philosophy, or practical facts. Layers of earth, even to seven feet in depth, can no more intercept the transmission of gas into the atmosphere than they can preclude the filtration of water. The power of the one is to descend and the other to ascend through a permeable medium. Water descends by its weight and affinity or cohesion. Gas

ascends by a double power; first, by the impulse given to each column of it in the focus of fermentation, from which it is evolved with such a force and elasticity that it requires a thick covering of lead and wood to check it in vaults; secondly, by atmospheric pressure, the action of which upon the surface of a grave 32 square feet, is equal to a weight of about 71,424 lbs. The only advantage in the depth of a grave is that of rendering septic formation more slow, on account of the slow diffusion of heat through the deep ground; the want of this is replaced in proportion to plentiful rains. We do not deny the gravity of gases under consideration, and that they do not rise in the air to a great height; but this is no protection against them: winds will not only raise but transport them to our dwellings. When such air, loaded with putrid emanations, remains stagnant, where it is little renewed, its effects become most deadly. Experience has over and over again shown that diseases of the worst kind, such as malignant, putrid, and exanthematous fevers are the fatal consequences. (See Dr. Felix Pascalis on Intra-mural burials.

"On the 17th of August, 1744, a body was conveyed to a vault in the Parish of Notre Dame, of Montpellier, attended by a numerous procession. While lowering the corpse, a man first went down to support the coffin, and fell senseless; another followed to assist him, and though drawn out in time, nearly perished; the third had the courage to descend with a rope around his waist, and had he not been drawn up immediately would inevitably have died; the fourth, a strong and vigorous man, trusting to a robust constitution, dared the danger, and died as soon as he had entered the vault; the fifth came out once to recover strength and returned the second time, staggered from the ladder and fell dead.

"Dr. Haguenot was commissioned to investigate the matter, and having had the vault re-opened, made the following experiments:

"1st. The cadaverous fetor was so tenacious as to adhere a long time to any substance which was left a few moments in the vault.

"2nd. Lighted tapers, chips, paper and tarred ropes, when brought to the edge of the vault, were instantly extinguished, as completely as if dipped in water.

"3d. Small animals, dogs and cats, became instantly convulsed

and died in a few minutes, birds only lived a few seconds on breathing the impure atmosphere.

"4th. The vapor or gas was caught in glass vessels, and, after being kept for six weeks, afforded the same qualities and produced the same results."

The whole of these were witnessed and certified by Dr. Haguenot and a committee of the faculty of the University of Montpellier.

G. A. Walker, author of the book previously referred to, declares that "of all the grave-diggers he has spoken to, not one has wholly escaped the effects of poisoning; many had been overpowered by the gases on commencing to dig."

At Paris, in 1852, three men died from inhaling an escape of gas from coffins, and Mr. Fourcroy declared that all the grave-diggers he had examined showed signs of slow poisoning.

Mr. Chadwick affirms "that the sexton's vocation entailed a loss of one-third of the natural duration of life."

When the coffin of Francis I was opened, at the end of the last century, the dreadful vapors drove the men back, though he had lain there 250 years.

In 1845 the burial pits closed up at Louis during the "Black Death," in the fourteenth century, were opened, and the men employed were overpowered by the smell.

It is said that Marylebone grave-yard, closed for more than thirty years, smells, as does also St. John's Wood.

Dr. Selmi, of Mantua, has taken the pains to bottle the air of some cemeteries in calm weather. He finds it to contain an organic corpuscle, which he terms "septa-pneuma." This, administered to a pigeon, in solution, developed putrid fever and destroyed the bird on the third day. (See "Cremation of the Dead," by Dr. Pietra, Paris, p. 9.)

In our city the old burying-ground before referred to has been discontinued for public burials since 1864. Dr. Rauch, Secretary of State Board of Health, in a pamphlet, published in 1866, maintained that this cemetery contributed to the contamination of the water supply of the city.

During epidemics of yellow fever and cholera, the pernicious influence of intra-mural interments in this country has been most striking. The epidemic in New York in 1822 was considered to

be the result of the overcrowded condition of Trinity churchyard cemetery before referred to.

Several instances are cited by Dr. Rauch, one of which was the epidemic at New Orleans in 1853. In the fourth district the mortality rose to 452 per thousand, which was more than double that of any other district in the city. In this district existed three extensive cemeteries, in which were buried the previous year more than three thousand bodies. In other districts the proximity to cemeteries seemed to aggravate the disease. Another instance was observed by Dr. Rauch personally during the epidemic of cholera in Burlington, Iowa, in 1850. No deaths occurred in the neighborhood of the city cemetery until about twenty interments had been made there, and then cases began to occur, and *always* in the direction from the cemetery in which the wind blew. Another instance was the epidemic at Norfolk and Portsmouth in 1855, reported by Dr. Bryant. Here 45 per cent. of the population died. Nearly all interments were made in the city, where the water-level is only six feet below the surface. The average depth of graves was about four feet, and, in many of them, three bodies were placed, one upon the other. These cemeteries were considered by Dr. Bryant a fruitful source of the disease.

In 1843, Edwin Chadwick presented to the Home Department a "Report on the Results of Special Inquiry into the Practice of Interment in Towns in the Kingdom of Great Britain and Ireland. Chadwick deduces, from the sum of evidence upon the subject, the conclusion: "That inasmuch as there appear to be no cases in which the emanations from human remains in an advanced stage of decomposition are not of a deleterious nature, so there is no case in which the liability to danger should be incurred, either by interment or by entombment in vaults, which is the most dangerous amidst the dwellings of the living; it being established, as a general conclusion, in respect to the physical circumstances of interment, from which *no adequate grounds of exception have been established*, that all interments in towns, where bodies decompose, contribute to the mass of atmospheric impurity which is injurious to the public health."

[TO BE CONTINUED.]

ARTICLE VIII.

A SIMPLE YET NOVEL WAY TO USE A CATHETER. By EDWARD C. HUSE, M.D., Rockford, Illinois.

The soft rubber catheters of Jacques and Nélaton, are a wondrous improvement over the old-fashioned urethral "spike" style of instrument. Doubtless hundreds of lives have already been saved by them which would have been sacrificed, as formerly, by the old barbarity.

We are all familiar with the bundle of one-eyed "spike" catheters, of varying sizes, in the show-case of every druggist, which, in fact, probably always will be seen there. Most of them appear to have been "in stock" a hundred years.

A device for using the soft rubber instrument to advantage, in a narrowed urethra was brought forward by Tiemann & Co., some time since, acting upon the suggestion of a well known professor in New York. The very absence of rigidity in the soft instrument, excludes its use in prostatic enlargement, if complicated with stricture. The "spike" instrument is of course highly objectionable unless the wire stylet be withdrawn, when it becomes, virtually, no better than the very soft ones.

The new design for stylet which Tiemann & Co. make, to be used on occasion, is undoubtedly an excellent thing. But if one will use the "spike," and, when it reaches the bulbo-membranous region (where it is apt to stop), will simply *withdraw* the stylet for an inch or a inch and a half, he will find substantially the same instrument in his hands as the new device, and that the rigid shaft, coupled with the soft, persuasive extremity are thus in his hands in a moment.

A case in point will illustrate this. Some two months since, O. B. S., aged 22, with a deep seated and tight stricture of three years standing, came to me for treatment. After dilating carefully for several weeks with *bougies à boule*, till a No. 6 could be got through, I resolved to use the *one-sitting* treatment, and accordingly operated with the divulsor of Thompson. There had been a prostatic abscess for somewhat over a year, the result, he

told me, of small and sharp bougies, Nos. 3 and 4, which he had *used upon himself*, after "reading up" on the subject. Some eight hours after the operation I withdrew a No. 12 bougie, which was introduced immediately after the divulsor had done its work, and attempted, by means of a "velvet eyed" soft catheter, to empty the bladder. It did not pass. The catheter, No. 12, was worked along, down to the prostatic region, and there stopped.

An experience of several thousand times passing the rigid silver instrument has shown me, as it ought to every one, that no *force* in such a case is ever admissible, or for that matter in *any* case where a catheter is used. *En passant*, how often this vital, or rather, mortal, principle is ignored! What a hecatomb of human sacrifice its disregard has cost! The vertebrated instrument of Squire did no better. What was needed was a rigid shaft with a *coaxing* end. Taking a No. 12 "spike," I went down carefully to the obstruction, withdrew, for an inch or so, the stylet, thus leaving a *climbing* or *roving* distal end to the instrument, pressed gently upon the shaft, and in a few moments a warm jet of urine showed me that the prostatic Rubicon was passed.

It is not easy, always, for country practitioners to get "velvet-eyed" or soft catheters, with complicated stylets and more complicated instructions. Especially is this true in winter, where muddy roads, ten foot snow banks or —40° place an embargo on the fulfillment of one's intention. It is just at this season that prostatic troubles are worst. For fifteen cents, and the simplest possible manipulation, many an accident, usually fatal, could have been and can be now avoided.

It is not my purpose, in conclusion, to claim anything startling or especially ingenious, in this article. It is simply a new adaptation of an old means to the fulfillment of a very important purpose, and thus the avoidance of a sunken rock, in our surgical navigation on which the life of many a patient and the reputation of many a physician, have been irretrievably stranded.

June 12, 1881.

Society Reports.

ARTICLE IX.

LANSING, June 13, 1881.

MEMBERS OF THE MICHIGAN STATE BOARD OF HEALTH,

Gentlemen:—At a meeting of the New Orleans Medical and Surgical Association, held May 7, the following resolution was adopted:

"*Resolved*, That the special attention of the National Board of Health be called to the unusual prevalence, at the present time, of typhus fever, scarlet fever, small-pox, and other contagious diseases in the Northern and in some of the Western States. We, therefore, urge the necessity of immediate action, the most rigid railroad inspection from such places, believing, as we do, that they may be considered *dangerously infected.*"

There have been reported from various portions of this State, cases of small-pox, so that now it may be considered to have been pretty well scattered over the State. So far, the local boards of health have succeeded in keeping the disease from spreading to an alarming extent. In this they have been aided by the fact that by action of this Board in years past, thousands of people in the State were vaccinated, and thousands more have been vaccinated this spring. But, although no general outbreak of the disease has occurred, yet, from the great numbers of immigrants pouring into and through this State, and the general prevalence of small-pox in foreign ports from whence these immigrants come, there is danger of such an outbreak, and it seems to me some action should be taken to prevent the further introduction of small-pox into the State. In order that there may be careful thought on this subject before action, I send you the copy

of the following letter from Stephen Smith, M.D., Committee of the National Board of Health on the Prevention of Small-Pox, and would respectfully suggest that you give this plan some thought, and be prepared to take some action at the regular meeting of the Board, July 12, 1881. The letter is as follows:

"*Dear Dr. Baker:*—The only plan by which small-pox, spread by emigrants, can be brought under effectual control, seems to me to be this, viz., the concerted action of municipal, State, and National sanitary authorities. The special functions of each might thus be stated: 1. The National Board might appoint Port Inspectors who should examine every immigrant, and give cards to each with name and date of inspection; if the person is found protected, give him a white card; if unprotected, vaccinate him and give him a red card with name and date of vaccination. 2. Appoint Inland Inspectors, to be located at the great centers where the emigrant trains pass from one State into another, as at Buffalo, Ogdensburg, Pittsburgh, Wheeling, Detroit, Cincinnati, Chicago, St. Paul, Kansas City, St. Louis. These inspectors could examine the cards, allow those holding white ones to pass, but re-examine those holding red ones, and if vaccination is not effectual, re-vaccinate. State authorities could inspect at such points as the emigrants are likely to be diverted from the train and locate in the State. 3. The municipal authorities would follow the emigrant to his home and secure general vaccination of the community.

"Would your State Board be interested in taking action in this matter, or, do you not suffer from immigrants? The National Board cannot properly act unless State and local boards call for aid. If there was combined action of the State Boards, the National Board could coöperate at ports and on inter-State lines, and the whole machinery of sanitary organization could work in harmony and effectually exterminate this pestilence. Emigrants arrive at New York at the rate of 6,000 and 7,000 daily—large numbers unvaccinated, and all have been exposed to small-pox in foreign parts. It seems to me that there is a grand opportunity for the sanitary authorities of the United States to combine, each working in its sphere, and give an example of their power to exterminate a plague which will increase for a year or more un-

less we make such an effort. I have detailed this plan to our Health Officer and Dr. Harris, and they approve. I think our State Board will ask the National Board to aid in preventing the introduction of small-pox.

(Signed) "STEPHEN SMITH."

Such an inspection service as detailed need not confine its work to small-pox, but the introduction of typhus fever and other contagious diseases could be guarded against.

I trust this important subject will receive due attention.

Very respectfully,

HENRY B. BAKER, *Secretary.*

ARTICLE X.

CHICAGO MEDICAL SOCIETY. Stated Meeting June 20, 1881. Dr. E. Ingals, President in the Chair. Dr. L. H. Montgomery, Secretary.

A valuable paper on "Cremation," read by Dr. Chas. W. Purdy, will be found in this number of the MEDICAL JOURNAL AND EXAMINER.

The discussion was opened by Dr. G. C. Paoli, who said, that probably the greatest opposition to the adoption of cremation would come from the Orthodox Church, although he failed to see any antagonistic principle mentioned in the Scriptures. Such opposition had generally been made to any progress of science. Hence he called on the newspapers to enlighten the public on the subject, and impress the people favorably, for it was evident that grave-yards were very harmful, and that cremation had become a necessity.

Dr. R. E. Starkweather thought that cremation was soon to be adopted. A prominent bishop of the Church of England had lately expressed an opinion that it was only a matter of time. It was not, however, so urgent in this country as in the crowded cities of Europe. Monuments raised to the memory of the dead, he thought, were not so commendable as the foundation of

charitable institutions and erection of buildings for that useful purpose.

Dr. C. T. Fenn thought the worship of the dead was so deeply rooted in the human heart that it was a question whether we could banish the idea that the dead is laid to sleep in the ground. He had no objection to have his body cremated, but he preferred to have those of his relatives and friends buried.

Dr. Valin said that the disposal of dead bodies was the mooted question. What shall we do with decomposing organic matter? He proposed to turn dead bodies into a source of revenue by selling them for dissection, and to soap manufacturers or fertilizing establishments.

Dr. E. Ingals did not approve of using the ashes of cremated bodies as fertilizers, but he knew of no better use to which old excavated bones could be put. A large quantity of such bones had come to the surface in the sand of old Lincoln Park grave-yard. He asked whether worms really infected dead bodies? He thought no living being could thrive in them. He referred to a pretty general fear among some people of being buried alive. Many people favored autopsies on that account. Many stories had been repeated of dead bodies having changed their position in the coffin; this was owing to the relaxation that followed rigor mortis, and should be explained to the laity. Besides a living being could not breathe in a common coffin, and could not live more than a few minutes. However, the body could not be considered dead before decomposition had commenced.

The decomposition by rotting or by cremating was the same, except that one process was much quicker. In order not to destroy criminal evidence some examination would probably be made. He did not believe there was any legal act preventing cremation. He disapproved of the extravagance generally displayed at funerals.

Dr. C. W. Purdy was convinced that the horror now manifested for incineration would be directed towards burying, as soon as the first had been witnessed a few times. He said worms did exist in corpses that decomposed with access to the air, but not in coffins underground. Yet serpents had been found in old coffins. He did not approve of the utilitarian theory. He professed a great respect for the dead, wichh he said should not be

diminished, since it was one of the noblest feelings of the human heart, and he proposed crémation as a much more respectful procedure than burying.

The Society adjourned to October next. H. D. V.

THE POISONOUS EFFECT OF INTRA-VENOUS INJECTIONS OF PANCREATIC MICROZYMES.

In a late communication to the French Academy of Sciences, A. Béchamp showed that all the known properties of the pancreas were dependent on its microzymes. Some experimenters, who had obtained toxic effects from the ferment of the pancreatic juice, desired to experiment with these microzymes, and see if their introduction into the organism would be followed by different results from those obtained in experimenting with pancreatine itself. They repeated their experiments with the microzymes of the liver, to have a point of comparison. They reached the following conclusions, which Dr. M. Raynaud read before the Academy of Medicine, March 8: 1. The injection into the blood, of isolated pancreatic microzymes, capable of digesting albuminoids, causes an almost instantaneous death, if their proportion is Og. 0001 for every kilogram in weight of the animal experimented upon (00 $\frac{1}{10,000,000}$ of its weight). It is impossible as yet to furnish a satisfactory explanation of the process of death, the only lesions observed being a congestion of the digestive mucous membrane, which ended in an effusion of blood, whenever death was relatively delayed. 2. The injection into the blood, of putrefied microzymes, which have evolved into bacteria, and are devoid of their fermentative power, does not give rise to any result. Hence the following important corollories: That death could not depend on embolism, which, beside, has never been found; that the bacteria replacing the pancreatic microzymes and the microzymes of fibrine, are absolutely harmless, and that a complete and radical change of function has been the result of the putrefaction experimentally induced in these micro-organisms. 3. The intra-venous injection with the microzymes of the liver is totally harmless, a fact which renders more wonderful the action of their congeners.

Foreign Correspondence.

ARTICLE XI.

VILLERS-SUR-MER,
NORMANDY, FRANCE, June, 1881.

MESSRS. EDITORS:—In crossing the sea with a large party of his own patients, a physician naturally interests himself in the symptoms of, and the effect of treatment upon, that invariable element of an ocean trip, sea-sickness. Dr. Beard, of New York, has lately written a pamphlet upon this complaint, and his chief weapon against its inroads is the potassium bromide, with which he advises physicians to intoxicate their patients before they go on board the steamer. This bromidism is then to be kept up during the passage, and thus, thinks Dr. Beard, sea-sickness may be kept at bay. I regret that I cannot endorse this opinion and this advice. In the first place I can hardly believe that any physician of calm judgment would place delicate women under such overwhelming influence of bromide for several days before they go to sea and continue it during the voyage. Secondly, I do not believe such treatment would overcome or prevent sea-sickness. Given in proper doses, while at sea, the bromides in my hands effected nothing whatever. Nitrate of amyl is another remedy suggested by others as well as by Dr. Beard. I used it in strong doses. It not only did no good whatever, but its penetrating, to many, nauseating, odor disturbed the stomachs of those who otherwise, perhaps, would not have been so ill. Dr. Beard thinks the galvanic current an excellent remedy in sea-sickness, but says its use at sea is impracticable. By cutting small openings in the cover of a Hall's French battery for the passage of the wires and afterward suspending the box, I

found no difficulty whatever in passing the Faradic current through the stomachs of my patients, the positive pole being placed in the epigastric region, the negative over the base of the brain. In one case this procedure seemed to effect a temporary relief. In all others it failed to accomplish any good whatsoever. The citrate of coffeine is also recommended. Its effects are *nil.* In short, I believe there are only two remedies for sea-sickness, and they are rather palliative than curative, but they do lessen the duration of the attack. One of these is *fresh air* taken in a supine position on deck, the other is indomitable *pluck.* If patients cannot be kept perfectly warm on deck it is better that they keep their berths. Cold at sea is extremely exhaustive. In one of my patients it caused an attack of neuralgia which exposed the lady to serious danger. Food of simple character, such as biscuit, oatmeal porridge, baked potatoes, well salted, and a little black coffee without sugar, will gradually accustom the stomach to food of more solid character. But the rule should be that if such simple nourishment be ejected by the stomach, a fresh quantity should at once be taken. It is not necessary to say that lemons, oranges (and perhaps apples), are indispensible.

A small mustard plaster over the epigastric region is sometimes a comfort, and hot water at the feet is a delight. But the *medical* treatment of sea-sickness is a snare and a delusion. It not only does no good, but I believe rather increases than assuages that feeling of unspeakable disgust, which led Charles Dickens to say on the first day out, that he was afraid he should die, and on the second, that he was afraid he should not.

In several voyages across the Atlantic I have as yet failed to meet a ship's surgeon who had any faith in medical remedies against sea-sickness. These gentlemen are as heretical in this direction as the veriest old sea-dog before the mast. They try to sympathize with sea-sick passengers but are quite ready to confess their absolute ignorance of effectual remedies. I am inclined to believe, however, that the doctors on ocean steamers are not overwhelmed by ambition. For the most part they are genial, pleasant fellows, ready and willing to do one a kindness. They go back and forth between two ports, year after year, quite satisfied, apparently, with their lot and their salary of four or five

hundred dollars. They are generally unmarried. Living costs them nothing. They have a good table, and their principal outlay is for the clothes they wear. So far as I can learn the majority of them are contented, or at any rate feel that they cannot do better than respond to the limited calls upon their professional skill at sea, and therefore do not attempt to secure anything better. It is a pleasure to feel that they do not expect to be feed at the end of the voyage. With the exception of the doctor and the captain and perhaps the first mate, I suppose there is no soul of the service of a sea steamer who will not accept a "tip" of any dimension. This is the one annoying drawback to the pleasure of the voyage. One of our humorous paragraphists recently said: "A first-class way to commit suicide by starvation is to live in a hotel and not fee the waiter." This strictly applies to a sojourn of ten days on an English ocean steamer. And, remember, unless you give at least ten shillings to the steward who has done nothing for you beyond changing your plates at table, he will not only not thank you in the slightest degree, but will become your enemy. It will not do to starve, for you have a European trip in mind, so you give your steward the ten shillings, or a sovereign, and feel disgusted at the mental degradation of English and European menials of every description. You are allowed to breathe in England and on the continent but hardly without a fee.

On this side of the water the American physician rather dreads to purchase medicines. He quickly appreciates the greater elegance of the preparations of leading druggists at home. If he wishes to procure a bottle of quinine pills he finds a rough looking article, moderate in price it is true, but bearing no comparison with our sugar-coated or our beautiful gelatine-covered pill. The English pill is covered with *French chalk*, of all substances. I was glad to find in London, after a prolonged search, the pills of McKesson & Robins. But the indications were that they were not much in demand.

In France a pill is usually coated with silver foil and is quite satisfactory. The general manipulation of the French druggist is much superior to that of the English pharmacist, but an American physician abroad is disappointed by his failure to find

the many handsome and neat preparations of the alkaloids which are so abundant in the United States. The cost of all medicines, however, is much lower in Europe than in our own country. For example, I purchased one-grain quinine pills at the rate of seventy-five cents the hundred. In London an ounce of the potassium chlorate may be purchased for five cents.

While in the latter city last month, I had the pleasure of making the acquaintance of Mr. Howard Marsh, an orthopœdic surgeon of great ability, and a gentleman whose charming courtesy I shall not soon forget. Aside from his cases in one of the large city hospitals he has a special hospital for hip-joint cases, supported by charity. Many of his methods of treatment were unique and original. I shall have more to say about them in a future letter. Mr. Marsh is a young man who holds a high place in the esteem of his London confrères, and I am sure will one day win a reputation which will extend beyond his own country.

Mr. William Adams, of London, made many warm friends among our surgeons during the International Medical Congress at Philadelphia, in 1876. During a call upon him he kindly showed me his plaster casts of the contracted finger cases which are beautifully shown in his recent work on this subject. In the billiard room of his house he has a case filled with these casts. They show the hands upon which he operated. Side by side are the deformed and the restored members. His success is very remarkable. His methods are so original and so simple as well, that I would earnestly advise those of your readers who are interested in the treatment of this deformity to purchase and study the Adams' book. It is published in reprint by Mr. Blakiston, of Philadelphia.

During a recent stay of some weeks at Ventnor in the Isle of Wight, I was strikingly reminded of San Remo, on the Mediterranean, a place which stands very high as a resort for consumptives. Ventnor has the same configuration, in a lesser degree, as San Remo. It is packed away at the base of high hills which protect it from northerly and northwesterly winds, and, therefore, possesses a climate which is milder than that of any other locality in England. English physicians send their chest cases to Vent-

nor for the winter. It offers many of the advantages of towns along the Mediterranean Sea, and is preferable to Nice and Cannes, because less subjected to sudden changes of temperature and to winds which blow across snow mountains.

It is a poem in the beauty of its setting; its rambling, elbow-jagging streets which wander hither and yon at their own sweet will; its charming villas, thatched cottages and lovely gardens. Verdure is so abundant that in a country walk one finds every porch, spire, post, tree, rock and fence buried beneath a deluge of ivy vine. And in the woods the very ground is a mass of ivy leaves. These vines cover and cling to every possible support. Picturesqueness is a word which describes the whole, from the stately hills and the bold cliffs on the shore, to the humble vine-clad cottage of the peasant. I am writing from a charming little town in Normandy. All about me are the same stone farm houses which are so common in Ventnor, with thatched roofs giving a holding place to thick moss, wild flowers and grasses. I am led to imagine, therefore, that the peasant's houses in the Isle of Wight were built after models left in the island by Normans and their descendants. In Ventnor is a noble charity in the shape of the Royal National Hospital for consumptives and diseases of the chest. Dr. Arthur Hill Hassam was its founder. It is supported by voluntary gift and by the small sum of $2.50 per week, which each patient is obliged to pay. The feature which renders this institution unique, is the plan of treating all patients on the "separate system." Every patient has his own chamber. This, of course, is accompanied by the mental advantage of sparing patients the depressing effect of the ward system of treating consumption. A large percentage of patients become restored to health, and the majority improve under the regimen of the hospital. The institution consists of eight blocks of two houses each, standing side by side. In the sleeping story these houses are only one room deep. Each room, therefore, has the same amount of sunlight. They face to the south and look out over an immense garden toward the sea. Patients are kept strictly to certain rules, which comprise directions for the bath, the hours passed in the open air and the diet. The grounds comprise twenty acres. Vegetables and milk are supplied by the farm.

No incurable cases are admitted. One hundred patients is the limit of the number received. Two additional houses, however, will soon be erected. Verandahs run the whole length of the hospital on the lower and second story, those on the ground floor being covered. So that patients may take fresh air even during rainy days. If space allowed, I would gladly give you details of the admirable success of this institution. It is one of the lions of the place, and Great Britain as well as Ventnor is proud of it.

I will ask your readers to bear in mind that Ventnor, Isle of Wight, is a place to which they may safely send those lung and heart cases which bid fair to improve under suitable change of climate. The real benefit to be derived from such change of course consists in the ability of patients to live out of doors several hours daily. To send incurable cases away from the comforts and surroundings of home has always seemed to me a needless cruelty. The cost of living in Ventnor during the months from October to June is quite reasonable. * *

ARTICLE XII.

VIENNA, June 16.

EDITORS CHICAGO MEDICAL JOURNAL AND EXAMINER:—The famous operation, the excision of the cancerous pylorus, has scarcely realized the bright anticipations of its creator, perhaps because the cases hitherto operated upon have not been judiciously selected. Some nine stomach resections have been already reported, but one of the patients still surviving. Even the first, Billroth's own, operated January 29, has recently (about May 26), succumbed to a recurrence of the disease. Besides these there have been doubtless several cases *not* reported, as I know personally in one instance which occurred in Vienna. It is of course to be expected that patients will wait long, usually too long, before submitting themselves to an operation so formidable. Yet in the Continental hospitals, where patients place implicit trust in the surgeon, opportunities will doubtless occur for a fair and thorough test of the value of this daring but after all rational innovation. The one case which has not yet been terminated

fatally, was that of Billroth's assistant, Woelfler, performed in April last, in which, according to the operator's statement, the malignant disease was confined absolutely to the stomach, all other organs, including the lymphatic glands, being apparently unaffected. As may be imagined, this patient will be watched with anxious interest by all friends of the operation. Woelfler has recently published a monograph, with plates, describing the operation and modifications as performed by Billroth, published by Braumueller & Co., Vienna.

Another novelty, less startling, but perhaps more valuable, has been within the last few months introduced and pretty thoroughly tested in Vienna; the local application of iodoform to unhealthy, particularly tuberculous, granulations. Brought out by Mosetig, it was soon used quite extensively in Billroth's wards, and with the most satisfactory results, according to the report of B.'s assistant, Mikuliez, at the surgical congress in Berlin. All indolent, granulating surfaces, but particularly the fungous granulations so often observed after chronic synovitis with ulceration of cartilage, have been observed to assume rapidly a healthy appearance, and exhibit an unusually favorable course after a few daily applications of iodoform crystals, so that this procedure has taken a recognized place in the armament of the Vienna surgeon.

By no means the least important of recent Vienna promulgations, are the researches of Dr. Jos. Gruenfeld, with the endoscope, or rather urethroscope. Ever since Désormeaux's presentation of his complicated endoscope in Paris, 1867, this branch of investigation has received more or less fitful and transitory attention from various specialists. Nearly all have contented themselves with the construction of instruments involving abstruse optical principles, culminating in the recently constructed, very beautiful, elaborate and expensive, but (for the urethra) comparatively worthless electrical endoscope of Nitze in Dresden, made by Leiter, in Vienna. Meanwhile, Dr. Gruenfeld, with very simple instruments, was training the *eye*. Beginning in 1871, as assistant in Sigmund's extensive syphilis division of the Vienna General Hospital, he has steadily and perseveringly pursued the study, and has finally, during the last year or two, by

means not simply of various publications on the subject, but also (and this is far more satisfactory) through many admirable demonstrations in the Royal Society of Physicians, earned deserved attention and recognition, not simply for himself and his method, but also for the subject—*urethroscopy*. This recognition took tangible shape in the form of an invitation by Billroth to write a volume for the "System of German Surgery" edited by Billroth and Luecke of Strassburg, now appearing. The book appeared last year, is being already translated into Italian, and will soon appear in English. That the author has somewhat exalted notions as to the future of endoscopy, is not unnatural; but that he gives valuable information as to the investigation and treatment of pathological conditions in that small but troublesome field, the urethra, is acknowledged even by his fiercest opponents and critics. I was so fortunate as to enjoy for several months Dr. G.'s private instruction, and propose in future letters to give the JOURNAL readers the benefit of my experience.

Another interesting and perhaps practically valuable work is the investigation of the origin and formation of vesical calculi, by the young but already well-known specialist, Dr. Robt. Ultzmann, of Vienna. The work has not yet been published, but will be presented to the International Congress, in London, in August, illustrated by numerous micro-photographs of sections through vesical stones. This work has especial value from the fact that the rapidly increasing popularity of lithotripsy à la Bigelow, is fast removing the possibility of convenient work in this line of investigation.

THE next meeting of the American Dermatological Association will be held at Newport, R. I., on the 30th and 31st of August and the 1st of September next. Papers are expected from many of the members, including Drs. White and Wigglesworth, of Boston; Duhring and Atkinson, of Philadelphia; Heitzman and Sherwell, of New York; Atkinson, of Baltimore; Hardaway, of St. Louis, and others.

Domestic Correspondence.

ARTICLE XIII.

FALLS CITY, NEB., June 6, 1881.

EDITORS MEDICAL JOURNAL AND EXAMINER: My attention was first called to the term, gangliasthenia, by an article in the *Michigan Medical News*, under date of April 11, 1881, written by Dr. E. Halsey Wood, of Hersey, Michigan; and according to this gentleman, "gangliasthenia means loss or diminution of ganglionic nerve power." "The term does not denote the condition of the ganglia, but expresses the fact that they do not generate a normal amount of nerve force."

Previous to the appearance of Dr. Wood's article I had seen and treated several cases of a peculiar nervous trouble, which seemed to be affections of the sympathetic system of nerves, as indicated by the symptoms.

In meeting with these cases, I was puzzled to know what to call them, and did not call them by any name, except that vague term "nervous trouble," which satisfies the people, and we can go on and treat. But this did not satisfy me, and ought not to satisfy any physician.

Upon looking for something on the subject, in our text books, I could find nothing in the books to which I had access; so I treated them according to common sense, and fulfilled the indications which the symptoms pointed out. Upon seeing Dr. Wood's article, it gave me a name which I thought was a good one for the trouble or disease I had met with, and which I think, will be plainly seen by the history of the following case:

On February 9, 1881, was called out five miles from town, to see a Mr. H. Upon asking the man who came for me (who was his brother), what seemed to be the trouble, he said his

brother was suffering with chills at the present, but had been sick for several months, and did not know what was the matter. He had been treated during that time, first by a couple of physicians from a neighboring town, and last by an eclectic physician from this place; but the patient did not improve. Mr. H.'s trouble had been called kidney disease, liver trouble, chills and ague, heart disease, dyspepsia, and his friends thought he was dying of consumption. Upon reaching the house, I found the patient to be a young man about twenty-five years of age; an American; had been married about seven months; a farmer; had been very healthy until about three months previous. The first symptom he noticed was his becoming very tired upon slight exercise; his back ached, he was languid; he did not rest well at night; was troubled with constipation, and with frequent frontal headache; his food was not digested well, and sometimes gave pain; his appetite was changeable; he became nervous and weak and began to lose flesh; was troubled with palpitation of the heart, slight cough, frequent flushes or rush of blood to different parts of the body, with a sensation of warmth or heat; frequent chilly sensations; localized sweating, such as one limb, the body, the hands or the head and face; insomnia; frightful dreams; blurred or disordered vision, congested conjunctivæ, convulsive closure of the extremities, and frequent epistaxis. These symptoms, slight at first, continued and gradually increased in severity. When I saw him, February 9, he was confined to his bed, suffering with se ere palpitation of the heart, one or two severe chills a day, without much fever after them, sometimes he would have quite high fever without any marked chill; he had pain over the heart and liver; headache; sleep very much disturbed, and so nervous that if a person should touch the bed-covers it would startle him, even if he was asleep; his back was very weak; his bowels very irregular, sometimes constipated and sometimes troubled with diarrhœa; at this time he had no localized sweating, but would sweat all over very profusely, from three to seven hours at a time; would wet the bed-clothes and sometimes through the mattress.

I examined his heart and lungs and found them healthy, except with slight bronchitis, his cough was sometimes dry and

sometimes with free expectoration. I examined his urine, and found his kidneys in a healthy state, but his urine was at times free and abundant, while at other times scanty.

I will now leave the readers of this article to draw their own conclusions as to the relation of the sympathetic system of nerves and the foregoing symptoms, and the propriety of calling this trouble "gangliasthenia."

As regards treatment. I first had him give up the use of strong tea, of which he drank three or four cups a day; also the use of tobacco, of which he used a great deal; had him take a tablespoonful each of Scott's emulsion of pure cod-liver oil and Trommer's extract of malt, three times a day, and bromide of ammonium and potassium, each seven grains, tr. ferri chlorid. eight drops, and tr. digitalis, six drops every three hours. Sulphate of atropia, 1-80 of a grain, to be taken when the profuse sweating began. When his bowels were costive or constipated to take a teaspoonful of the following, night and morning:

℞. Fl. ext. cascaræ sagradæ.
Tr. aloes et myrrh.
Syr. tolu, aa ℥j.
M.

Saw the patient again on the 15th, and found him much better; he felt stronger, was not so nervous, the chills were not so frequent nor so severe; his heart did not palpitate as much; his head ached very little; did not sweat so much; rested a little better at night, and did not dream so much; his nose did not bleed as often; appetite still poor; vision blurred; congested conjunctiva; still convulsive closure of the eye-lids; coldness of extremities and flushes of heat less frequent; cough no better. Continued the treatment with the addition of ten grains of chloral hydrate at bed-time.

March 2, was called to see Mr. H.; the family thought he was dying. I found him very much excited, respirations forty-eight in a minute, with considerable pain in the chest, skin dry and extremities cold. Gave him some brandy, with about twenty drops of fl. ext. of jaborandi, and had his feet bathed in hot mustard water. In about fifteen or twenty minutes he began to perspire quite freely; his respirations soon came down to twenty-four per minute. For two or three days after, cough was worse

and the sputa were rust colored, and the first day streaked with blood, indicating more or less congestion of the lungs. Continued the same treatment with an increase of ammonium bromide to ten grains. I did not see him again until March 24, at which time his cough had left; he could sit up all day; his appetite was better but digestion painful; his bowels were regular; profuse sweating had ceased; still had chilliness, but no marked chills; rested well at night, had no bad dreams nor headache; had no convulsive closure of the eyelids, vision was good; the hot flushes were very slight; had no palpitation of the heart; was still nervous and weak. Continued the oil and malt, gave citrate of quinine and iron ten grains, bromide of ammonium ten grains, and potassium bromide five grains three times a day, ten grains of lactopeptine after each meal, and cold salt water baths every other day, followed by rubbing with a coarse towel. This treatment was continued until April 5, when he was feeling about well, was able to be out of doors, but was still feeling weak. I withdrew all medicine, except the oil and citrate of quinine and iron, which he took twice a day, until April 15, when all medication was stopped, and to-day he feels better than he did before he became sick, but not as strong.

A. B. NEWKIRK, JR., M.D.

ARTICLE XIV.

EDITORS CHICAGO MEDICAL JOURNAL AND EXAMINER:—On April 17 I delivered Mrs. Rebecca Denning of an eleven-pound boy—full time. Mrs. D. is a native of Clay county, Indiana, where she was born the 13th day of January, 1827. She was fifty-four years three months and four days old at the time of her last delivery. She is the mother of thirteen children. She is a large woman, weighing about 200 pounds, of a sanguine, lymphatic temperament. Mr. John Denning, her husband, is sixty-four years past, hale and active, physically and mentally.

Yours cordially,

STREATOR, ILL., May 14, 1881. JNO. J. TAYLOR.

ARTICLE XV.

EDITORS CHICAGO MEDICAL JOURNAL AND EXAMINER:—I was delighted to read the excellent paper of Dr. Clevenger in the May number of your splendid journal, on the labors of my old friend and classmate, Dr. Schmidt. Allow me to mention another piece of ingenious mechanism of his I saw utilized for a most excellent purpose, whilst I was taking my degree course in the Medical Department of University of Pennsylvania, whilst the Doctor was prosector for Dr. Leidy, which to me is entirely unique.

He took an articulated skeleton, and I am not sure but that he made that also, and on one side he sawed in two the bones of the extremities and fastened them together with a screw, and then made muscles of india rubber and attached them naturally, so by un-screwing it represented a fracture, and demonstrated the muscles causing the deformity; on the other side of the same skeleton were like muscles to show the deformity of luxations; this was for Dr. H. H. Smith, Professor of Surgery, and on me at least, they made a lasting impression, as I have never forgotten them.

The last time I had the pleasure of seeing the Doctor was in Selma, just after the surrender. I too had been Assistant Surgeon in the Confederate army, and like the Doctor, reduced from affluence to poverty. I was living in Alabama, my native State, not long afterward removing to this State, and I had lost sight of the Doctor, until reading Dr. Clevenger's paper, before referred to. All honor and a long life to the Doctor, for his devotion to our noble profession.

R. FOWLER, M.D.

AURORA, TEXAS, May 21, 1881.

PROFESSOR I. N. DANFORTH, of Chicago, has recently received from his Alma Mater, Dartmouth, the honorary degree of Master in Arts, a distinction which he has long deserved and which will in the future be succeeded by higher honors, if they who know him are not mistaken.

Reviews and Book Notices.

ARTICLE XVI.—SEWER GAS AND ITS DANGERS. By George Preston Brown. Published by Jansen, McClurg & Co., Chicago.

This little book is well worth reading, if for no other reason that it may impress upon the mind of the reader the extent to which the drainage of this city is defective. The author pursued his investigations in conjunction with the Chicago Public Health Department. His personal investigations have been very extensive, and therefore, although not a physician, his observations and many of his conclusions are of value. There is no way of correcting the existing evil of defective drainage, except by spreading a general knowledge of the chief faults in the present systems, and the best modes of correcting them. This is the object of the little volume under review. Fully one-half of the contents is taken up with descriptions of house drains which were allowing the escape of gas, that was plainly one, if not the chief, cause of the diseases which attacked the inhabitants. The faults pointed out, every reader will see, exist in a very large number of the Chicago houses. The conclusions which Mr. Brown arrives at in regard to the requirements of a perfect system it may be well to copy here.

1. There must be an unobstructed and positive ventilation of all the pipes and drains within and beneath the house.

2. The ordinary earthenware drains must be discarded, since they cannot be secured against breaking and defective joints.

3. Perfect joints and not mere connections must be made between sections of drains and pipes.

4. The waste-pipes and drains must be one continuous system, and made of material that will not break, leak, corrode nor

obstruct the free passage of the minutest particles which enter them.

5. Traps must be used that will not permit the escape of gas by defect in construction or use; or, better still, they should be entirely discarded.

All should remember that the best disinfectant and the best insurance against disease is pure, fresh air.

RACHITIS AND CALCIUM PHOSPHATE. By Dr. Des Vallères.

The introduction of phosphate of lime in the materia medica is not recent. On perusing old pharmacopœia, we repeatedly meet formulæ containing burnt hartshorn, lobster's eyes, oyster shells, *album græcum*, etc., which consist almost entirely of calcium phosphate. Physiological experiments lately performed with that substance, by Drs. Chossat, Lassaigne, Cazalis, Guérin, Piorry, Gosselin, and many others, gave results which justified the reputation it held in the opinion of our predecessors.

The properties of calcium phosphate, brought to light by the learned investigators above referred to, may be thus summed up: *It has an immediate effect on the consolidation and the reparation of bones; at the same time, and above all, it acts as an excitant of assimilation*—an important property, for what we digest and absorb, not what is ingested, nourishes the body. An idea, which could hardly fail to suggest itself, and it is a happy one, consists in associating peptine with calcium phosphate, after this has been rendered soluble and been changed into chlorhydrophosphate. I have been very much satisfied with it personally, and I have seen very good results follow its use, in puny children, lacking in energy, suffering from obstinate eye and ear troubles, from adenitis, very much predisposed to colds, etc. In a word, in all the sufferers from a scrofulous diathesis.

On the same principle, there is no doubt that phosphated peptone should be prescribed for pregnant women, who must furnish the fœtus with the necessary phosphates, beside its other elements. It is a well-known fact that, during gestation, the

bones of the mother lose in their firmness; something, not so well known, is the report of Dr. Pégot-Ogier on that point. Here are the conclusions at which he arrived:

It results from the use of calcium phosphate that:

"1. In pregnant women, most of the common ailments pertaining to their condition disappear, and the number of still-births is diminished.

"2. The mother's milk, often deficient in calcario-phosphatic elements, attains its maximum of richness which nature has fixed for the child.

"3. During infancy, youth, and until adult age, the development of the body progresses regularly; lymphatic nor rachitic diseases need be feared.

"4. The death rate, which, in Paris, is one to three (during the first year), has been brought down to one in five, which is the rate observed in the most salubrious places of the country."

Poor and sickly nursing children are common indeed; this most often is owing to one of two causes; either the mother's milk is too poor, or it is not well assimilated, the immediate result being an impaired digestion, which is often manifested by chronic diarrhœas, with wasting, and a slow alteration of the economy, which may lead to rachitis. In that case, nothing is so rational as the administration of phosphated peptones to the nurse, and this will bring the milk to its normal standard, and furnish the child with the stimulant of nutrition, besides the elements necessary for the formation of the bones and teeth.

According to Liebeg and Humboldt, crude calcareous mixtures are administered to children to facilitate their *growth* and their *dentition*, in Germany.

Indeed, I might insist on the benefits which we can expect from that preparation in all diseases of the bones: caries, necroses, osteo-malacia, Pott's disease, etc.; but I could not omit that to these diseases in particular does Trousseau's recommendation apply: "Choose especially the time when the constituents of the child's body are undergoing rapid changes, to renovate its constitution."—*L'Union Médical*, March 1.

SOCIETY MEETINGS.

Chicago Medical Society—Mondays, Aug. 4-18.
West Chicago Medical Society—Mondays, Aug. 11-25.
Biological Society—Wednesday, Aug. 6.

CLINICS.

MONDAY.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Prof. Holmes; 3 p. m., Otological, by Prof. Jones.
Mercy Hospital—2 p. m., Surgical, by Prof. Andrews.
Woman's Medical College—2 p. m., Dermatological and Venereal, by Prof. Maynard; 3 p. m., Diseases of the Chest, Prof. Ingals.

TUESDAY.

Rush Medical College—3 p. m., Dermatological and Venereal, by Prof. Hyde.
Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.
Mercy Hospital—2 p. m., Medical, by Prof. Quine.

WEDNESDAY.

Chicago Medical College—2 p. m., Eye and Ear, by Prof. Jones.
Rush Medical College—2 p. m., Medical, by Dr. Bridge; 3 p. m., Ophthalmological and Otological, by Prof. Holmes; 3:30 to 4:30 p. m., Diseases of the Chest, by Dr. E. Fletcher Ingals.

THURSDAY.

Chicago Medical College—2 p. m., Gynæcological, by Prof. Jenks.
Rush Medical College—2 p. m., Diseases of Children, by Dr. Knox; 3 p. m., Diseases of the Nervous System, by Prof. Lyman.
Eye and Ear Infirmary—2 p. m., Ophthalmological, by Dr. Hotz.
Woman's Medical College—3 p. m., Surgical, by Prof. Owens.

FRIDAY.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.
Mercy Hospital—2 p. m., Medical, by Prof. Davis.

SATURDAY.

Rush Medical College—2 p. m., Surgical, by Prof. Gunn; 3 p. m., Orthopœdic, by Prof. Owens.
Chicago Medical College—2 p. m., Surgical, by Prof. Isham; 3 p. m., Neurological, by Prof. Jewell.
Woman's Medical College—11 a. m., Ophthalmological, by Prof. Montgomery; 2 p. m., Gynæcological, by Prof. Fitch.
Daily Clinics, from 2 to 4 p. m., at the Central Free Dispensary, and at the South Side Dispensary.